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CHEMISTRY OF THE SUN

THE
CHEMISTRY OF THE SUN

BY

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> "——Si quid novisti rectius istis,
> Candidus imperti; si non, his utere mecum."
> HORACE, *Epist.* Liber. i—v

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PREFACE.

"We do not know that any one of the bodies denominated elementary is absolutely indecomposable."—DALTON.

"It is conceivable that the various kinds of matters, now recognised in different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."—GRAHAM'S *Researches*, p. 299.

IT is because the secrets of the Sun include the cipher in which the light messages from external Nature in all its vastness are written, that those interested in the "new learning," as the Chemistry of Space may certainly be considered, are so anxious to get at and possess them.

I purpose to show in the following pages that even if centuries must elapse before the ingenuity of man will succeed in doing for Celestial hieroglyphics what it has already done for Egyptian and Assyrian ones, in one direction at least an alphabet is already being formulated.

The attempts which are now being made to investigate the nature of the Sun, may be divided into three perfectly distinct branches. We have, first, that extremely important inquiry which has as its result the complete determination of the position of everything which happens on the sun.

This, of course, includes a complete cataloguing of the spots which have been observed time out of mind, and also of those solar prominences the means of observing which have not been so long within our reach. It is of the highest importance that these data should be accumulated, more especially because it has been found that both in the case of spots and prominences there are distinct cycles which, in the future, may not only be very much fuller of meaning to us than they seem to be at present, but may even satisfy the representatives of the *cui bono* school.

The second branch of the work is this:—These various cycles of the spots and prominences have long occupied the attention both of meteorologists and electricians; and one of the most interesting fields of modern inquiry, a field in which very considerable activity has been displayed in the last few years, is one which seeks to connect these various indications of changes in the sun with changes in our own atmosphere.

The sun, of course, is the only variable that we have. Taking the old view of the elements, we have fire represented by the sun, variable if our sun is variable; earth, air, and water, in this planet of ours, we must recognise as constants. From this point of view, therefore, it is not at all to be wondered at that both electricians and meteorologists should have already traced home to solar changes a great many of the changes with which we are more familiar. This second line of activity depends obviously upon the work done in the first, which records the number (the increasing or decreasing number) of the spots and prominences, and the variations in the positions which these phenomena occupy on the surface of the sun. As a result of this work, then, we shall have a complete cataloguing of everything on the sun, and a complete comparison of everything which changes on the sun with every meteorological phenomenon which is changeable in our planet.

We next come to the third branch of the work, the newest parallel in the quiet sap now going on; this has to do with solar chemistry.

The attempt to investigate the chemistry of the sun, independently even of the physical problems which are, and indeed must be, connected with such an inquiry; is an attempt almost to do the impossible, unless a very considerable amount of time and a very considerable number of men be engaged upon the work. If we can get as many investigators to take up questions dealing with this subject as we find already in other branches of knowledge more closely connected with the old curriculum of studies, we may be certain that the future advance of our knowledge of the sun will be associated with a future advance of very many of those very problems which at the present moment seem absolutely disconnected, and indeed distract attention, from it.

I have, in the present volume, to limit myself to this chemical branch of the inquiry. Here, as in other regions of physical and chemical research, advance depends largely upon the improved methods which all divisions of science are now placing at the disposal of all others. Our knowledge of the chemical nature of the sun is now being as much advanced by photography, for instance, as that descriptive work to which I have already referred, which deals with the chronicling and location of the various phenomena.

One of the advantages which has come from the introduction of new apparatus has been the possibility of making maps of the solar lines and of the metallic lines which have to be compared with them on a very large scale. Thanks to the generosity of Mr. Rutherfurd and the skill of Professor Rowland, magnificent diffraction-gratings have been spread broadcast among workers in science, and we have therefore easy means of obtaining with inexpensive apparatus a spectrum of the sun, and of mapping it on such a scale that the fine line of light which is allowed to come through the slit is drawn out into a band or spectrum half a furlong long. A complete spectrum on this scale, when complete (as I hope it some day will be, though certainly not in our time), from the ultra-violet to the ultra-red, will be 315 feet long, on the scale on which I have already been working. This is a considerable scale to apply to the investigation of these problems; but recent work has shown that, gigantic as the scale is, it is really not beyond what is required for honest, patient work.

I announced some years ago to the Royal Society that, reasoning from the phenomena presented to us in the spectroscope when known compounds are decomposed, I had obtained strong evidence that the so-called elementary bodies are in reality compound ones.

The idea of simplifying the elements is connected with the philosopher's stone; the use of the philosopher's stone was to transmute metals; therefore I was at first supposed to be "transmuting" metals; and imaginations had been so active in this direction that I am not sure that when one of my papers was read at the Royal Society, many were not disappointed that I did not incontinently then and there "transmute" a ton of lead into a ton of gold.

Hence there have been misapprehensions of the nature of my work, to say nothing of some denunciations of it on the part of those who have not taken the trouble to inform themselves concerning it; and in this I have a reason for bringing together in the present volume the points touching both the origin of the views I have advanced and the work which has led up to them.

It is now upwards of seventeen years since I began a series of observations having for their object the determination of the chemical constitution of the atmosphere of the sun. The work which had this object merely, in the first instance, has opened up a great number of problems above and beyond the initial question, because we were dealing with elements under conditions which it is impossible to represent and experiment on here

In the first place, the temperature of the sun is beyond all definition; secondly, the vapours are not confined; and thirdly, there is an enormous number of them all mixed together, and free, as it were, to find their own level. Nor is this all. Astronomers have not only determined that the sun is a star, and have approximately fixed his place in nature as regards size and brilliancy, but they have compared its spectrum with those of the other stars which people space.

This, then, is one branch of the inquiry, which has consisted in a careful chronicling of the spectroscopic phenomena presented to our study by the various stars.

Experimentalists have observed the spectrum of hydrogen, of calcium and so forth, in their laboratories, and have compared the bright lines visible in the spectra with the dark ones in the stars, and on this ground they have announced the discovery of calcium in the sun or of hydrogen in Sirius.

In all this work they have taken for granted that in the spectrum thus produced in their laboratories, they have been dealing with the vibration of one specific thing, call it atom, molecule, or what you will; that the vibrations of these specific molecules have produced all the lines visible, which they have persistently seen and mapped in each instance.

Now, it was not long before my work raised the question whether what has been thus taken for granted is really true. And now that the question is raised, the striking thing about it is that it was not asked long ago.

Time out of mind—or, rather, ever since Nicolas le Fèvre, who was sent over here by the French king at the request of our English one at the time the Royal Society was established, pointed out that chemistry was the art of *separations* as well as of *transmutations*—it has been recognised that with every increase of temperature, or dissociating power, bodies were separated from each other. In this way Priestley, from his "plomb rouge" separated oxygen, and Davy from potass separated potassium; and as a final result of the labour of generations of chemists, the millionfold chemical complexity of natural bodies in the three kingdoms of nature has been reduced by separations till only some seventy so-called elements are left.

Now this magnificent simplification has been brought about by the employment of moderate temperatures—moderate, that is to say, in comparison with the transcendental dissociating energies of electricity available in our modern voltaic arcs and electric sparks.

But, in the observations made during the last thirty years on the spectra of bodies rendered incandescent by electricity, *we have actually, though yet scarcely consciously, been employing these transcendental temperatures;* and if it be that this higher grade of temperature does what all other lower grades have done, then the spectrum we have observed in each case is not the record of the vibrations of the particular substance which we have put into the arc and with which we have imagined ourselves to be working alone, but of all the simpler substances or forms of the same substance produced by the short or long series of the "separations" effected.

The question, then, it will be seen, is an appeal to the law of continuity, nothing more and nothing less. Is a temperature higher than any yet applied to act in the same way as each higher temperature which has hitherto been applied has done? Or is there to be some unexplained break in the uniformity of nature's processes?

I give in the present volume the reasons which compel me to hold that the answer to the question put is, that these transcendental temperatures *do* dissociate, and that therefore what has hitherto been taken for granted is, in all probability, *not* true, and I attempt to show that both in observatory and laboratory the spectroscopic phenomena observed are simply and sufficiently explained on the view that the so-called chemical elements behave after the manner of compound bodies.

The views which I have put forward, as being what I honestly believe to be the true outcome of the seventeen years' work which I have done, depend for their strength upon the convergence of very various lines of thought and work, and I show in a concluding chapter that they appear to correlate the various physical phenomena presented by the sun's surface.

No doubt the future progress of science will show that we, after all, are looking through a glass darkly, and are not yet face to face with the whole truth. We must all of us be content to have our work criticised and expanded by future work; by researches carried on with greater skill; with more elaborate methods and higher views; while it is certain that we shall get a much higher and much richer truth out of further inquiries.

It is necessary that I should state in conclusion, that without the aid afforded by the organisation of the Committee on Solar Physics, the observations on sun-spot spectra referred to in chapter xxiii., and on which are based the views detailed in the concluding one, could never have been made.

J. NORMAN LOCKYER.

December, 1886.

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ERRATA.

Page 174, line 9 from top, *for* "187" *read* "1874."
,, 270. 4 ,, ,, "limits," ,, "cases.'

THE CHEMISTRY OF THE SUN.

CHAPTER I.

INTRODUCTORY.—KEPLER, NEWTON, WOLLASTON, 1600–1802.

THE field of research which I propose to treat in this book is one which has been opened up so recently that it may be said that the spectroscope, the instrument employed in the work, is to the men of science of to-day what the telescope was to Galileo and Fabricius.

This being so, although great strides have undoubtedly been made during the last twenty years, it must not be expected that we can do more yet than get an idea of the grandeur of the problems that will present themselves for notice to those who come after us. We are as yet only in the position of those who pick up pebbles on the shore.

How comes it, then, that in these later days we have been able to add the study of the sun's chemistry to the study of its form, size, distance and physical appearance? We have done this by adding the study of the molecular to the study of the molar. Relegating the sun taken as a whole to the disciples of the older school of astronomers, we deal with its minutest particles, and confine our attention to the unravelling of the tangled skein of light which each one of those particles sends to us. Hence we have two distinct fields of work—one molar, and here the telescope is supreme; another molecular, and here the spectroscope is our guide and helper in a region full of marvels.

What I have first to do, then, is to show on what principles our inquiry in the new field of research to which I have alluded and of which this book treats, is based; how it comes that the spectroscope has taken its place among the most important instruments employed in scientific work; what special qualities of light it utilizes and how it utilizes them.

And first as to the nature of light itself. In dealing with the phenomena of light, or to put it in broader terms, with the total radiant energy given out by the sun, or, indeed, by any other light source, there is an important consideration to be borne in mind in the first instance.

The actions involved in sending a telegraphic message may help us to form a correct mental image of what goes on when the sensation of light is produced. We have first of all a sending instrument, next a medium—a wire along which the message travels—and finally a receiving instrument. So also in the case of light.

Dealing first with the light source—the analogue of the sending instrument—experiment and analogy have abundantly proved that that which we call light has its origin in molecules of matter in a state of agitation or vibration, and the quality of the light radiated depends upon the inherent nature of the molecule and upon the quality of the energy which sets it in vibration or controls its vibration.

Among our most familiar light sources are the sun, the moon, the stars, gas and candles; and it will be well to remark here, and the reason why will be shown in the sequel, that the light which all these sources give to us is white light in the main.

Next as to the medium. To account for the transmission of light from the source to the receiver, say the eye of the observer, physicists have to assume the existence of an all-pervading imponderable fluid inappreciable to any of our senses. This they call Ether, and, according to the hypothesis, the vibrating molecules of the luminous body impart their vibrations to the ether, by which they are transmitted in the form of waves that travel with an inconceivable velocity to the receiver. The more quickly the molecules are vibrating the greater will be the number of the waves generated in the ether in unit time—say one second; and as the velocity with which the waves are propagated is the same, or at all events not widely different, for all kinds of waves (about 186,000 miles a second) it follows that the length of each wave will depend upon the rate of vibration of the molecules of the luminous body.

Turning now to receivers, we find they fall into three classes. There is first that marvellous instrument—the human eye. There is next—also a very marvellous thing—the photographic plate. Both of these are very largely used by the spectroscopist and their importance cannot be overrated. And then, finally, making up our third class of receivers, we have everything else in nature. But although receivers of this class influence largely, and are largely influenced by, the light they receive, they are of comparatively small importance in spectroscopic study.

In some such way as this, then, we may roughly image to ourselves how bodies become visible to us, how they impress themselves upon our consciousness through the eye.

All bodies, whether far or near, are visible to us by means of their unrest. No motion of particles, no light. If all the bodies in space were absolutely tranquil we should never see them. But the normal condition of everything in nature is a state of most beautiful and exquisite unrest. Scientific men call this a state of vibration; but we need not quarrel about terms. Everything in Nature, far or near, is in this state of unrest, and if it were not so there would be for us no External World. From every material substance, including all distant worlds, the vibrations of their smallest particles or of their largest masses come to us along a medium which scientific men call ether, not that they know all about it, but because it is necessary, in order that their work may go on at all, that they should assume that there is something infinitely finer than matter, and not at all like the attenuated matter which pervades all space. This ether forms the highway along which the vibrations due to the state of unrest of matter travel to our eyes whence they are conveyed by a new channel to our brains, thus begetting in our consciousness the impression of the material world.

It is thus that modern science explains a truth known in its most general form as early as the time of Plato, who in his *Timæus* writes that there is no light without fire.

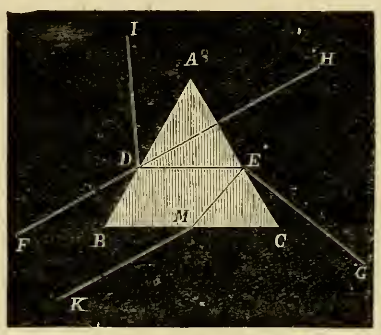

FIG. 1.—Copy of Kepler's diagram.

In fact, all light is originally produced by the vibration of particles under the influence of heat. Heat somewhere, whether in the sun or a candle or an electric spark, is the producer; reflecting surfaces anywhere, whether they be clouds in Jupiter or a tree or a ceiling, are the distributors.

We have now, then, got our light and the cause of it.

We next come to the question, What special quality of the light is it which we have to utilize in our study of the chemistry either of this or of distant worlds?

This light is an excessively complex thing, each sunbeam or candlebeam is, as I have said before, a very tangled skein, and our knowledge comes from the unravelling of it, which at once lays bare what we may term its structure.

This unravelling was first described, so far as I know, by Kepler, who used a three-sided prism, that is, a piece of glass shaped as in the diagram, Fig. 1. In his *Dioptrics* he shows that if a beam of sunlight be allowed to fall on such a prism in a certain way it passes out as a coloured beam—the colours of the rainbow being thus artificially reproduced.

As Newton is generally supposed to have discovered this effect, I give in a note Kepler's own words, from his work entitled *Dioptrice, sive demonstratio eorum quæ visui et visibilibus propter conspicilla, hoc est, vitra seu crystallos pellucidos, accidunt.*[1]

A period of 150 years now elapsed, during which nothing more, that we know of, was done in this matter; till Newton took up and extended the researches of Kepler, and by reason and experiment greatly enlarged our knowledge. Indeed, from his labours, the birth of spectrum analysis may be said to date.

Newton made a series of experiments which have since become of classic interest. He allowed a beam of sunlight to enter a dark room through a hole in a shutter, and strike on a prism so placed that its edges were horizontal and also that the beam entered it obliquely by one of its surfaces. He then received the light on a screen, and saw, as Kepler had seen before him, a band of colours arranged in the same order as those of the rainbow. In this experiment, if the base of the prism be upwards, the lowest colour will be red, next above orange, passing by imperceptible gradations to yellow, and afterwards to green, which then passes through the shades of greenish blue till it becomes a pure blue, then indigo, finally ending with a violet colour. The transition

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[1] "XVI.—Colores iridis jucundissimi oriuntur cum refractio est tanta; idque tam si oculi transpiciant quam si Sol transluceat.

"XVII.—Sole prisma irradiante tria genera radiorum resultant. Sincerus, vitri colore, et iridis coloribus.

"Sit enim F Sol. Is radiet in D. Hic quasi dividitur radii solaris densitas, quæ minima sui parte repercutitur in $D I$, et anguli, $A D I$, equali ipsi $B D F$ quo illabitur. Sincerum igitur radium, sed tenuem per $D I$ vibrat in I. Sincerus est, quia in vitro tinctus non est, cujus corpus non ingreditur.

"Potior autem pars de densitate ipsius $F D$ penetrat D et refringitur in $D E$. In E vero rursum dividitur, ratione densitatis. Potior enim pars transit E, et propter geminam magnam refractionem colores iridis jaculatur in G.

"Residuum ipsius $D E$ tenue admodum repercutitur a superficie $A C$ in $E M$: quod si $D E$ paulo obliquius in $A E$ incidit obliquius igitur in $E M$, refringitur quam hic. Nam si minuas $D E A$ erit et minuendus $M E C$, ex lege repercussus. Et sic denique $E M$ in $B C$ rectus incidet, itaque nihil in M refringetur. Cum autem $F D$ hoc pacto bis pertransierit corpus vitri, quippe semel in $D E$, iterum in $E M$, exiens recta per M, radium vitri colore jaculatur in K rectius tamen e regione ipsius A. Nam docemur ex opticis, radios lucides tingi in mediis coloratis."

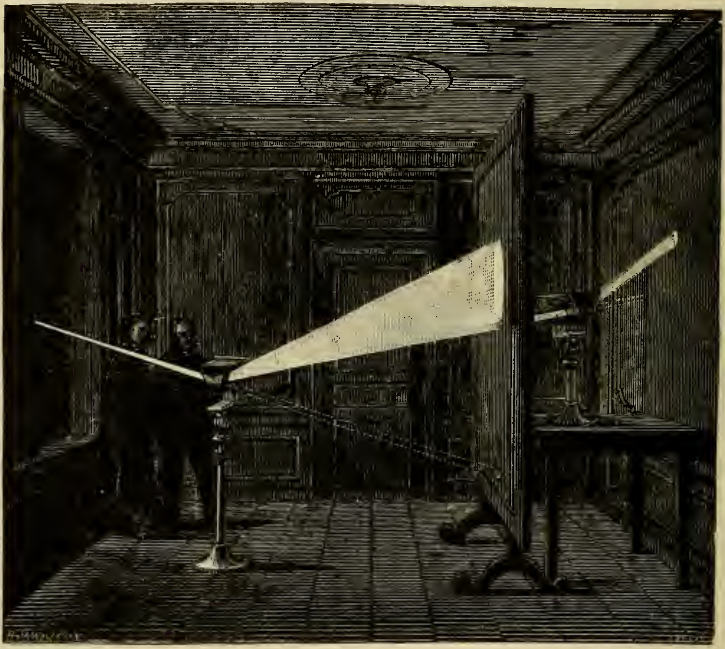

FIG. 2.—Decomposition of light by the prism. Unequal refrangibility of the colours of the spectrum.

from one colour to another is not abrupt, but is made in an imperceptible manner, so that it can scarcely be said, for instance, where the yellow ends or the green begins.

Next came the further experiments to which so much interest attaches. Newton, by making a hole in the screen, allowed one of the colours thus produced—say red—to pass on and through a second prism, receiving the image on a second screen: *the colour remained unaltered.*

This experiment proved that the rays producing the red colour of the spectrum so experimented on, were simple. The same result was soon found for all the others.

As Newton in his experiment operated with sunlight, the band of colours was in this case called *the solar spectrum;* and, indeed, the rainbow is nothing more nor less than a solar spectrum, caused by rain-drops playing the part of so many prisms. This being so, Newton could deal with the components of the light instead of with the light as a whole.

He then took two beams of differently coloured light—red and blue—and passed these two beams of different colour through the same prism. The action of the prism on these two differently coloured beams was found to be unequal; in other words, he got the red beam deflected to a certain distance from a straight line, and the blue deflected to a certain other distance. This experiment showed that there is a distinct difference in the amount of refrangibility, or, in other words, in the degree to which the original beam is deviated out of its course by its passage through the prism—that the red light is not diverted so far out of its original direction by the prism as the blue light is.

This leads us to Newton's first proposition, which is this:—"*Lights which differ in colour differ in refrangibility.*" This may be translated as follows:—"Lights which differ in colour are differently acted upon by a prism."

We now reach that proposition of Newton's which makes his work so germane to the purpose of this book:—"*The light of the sun consists of rays differently refrangible;*" that is to say, if, instead of using two coloured beams as in the last experiment, we take a beam of sunlight, we find that the *white light itself is compounded of light of different degrees of refrangibility.*

But how is it possible to show the truth of Newton's assertion, that white light is compounded of these different colours? We can do so by simply placing in the path of the coloured beam another prism placed in a contrary direction, as shown in Fig. 3. It will be seen that we get white light back again; for the second prism exactly undoes the work of the first.

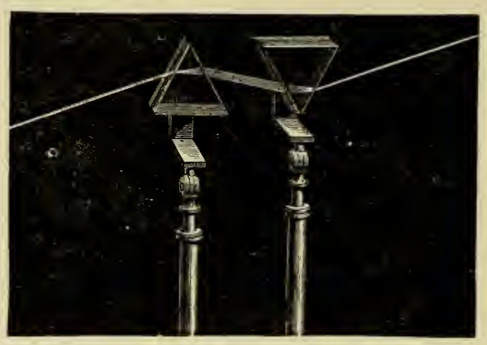

FIG. 3.—Recomposition of white light by means of a second prism.

The idea of white light, then, is simply an idea built up by the brain, because there is a multitude of light waves of all colours perpetually pouring into the eye with a velocity much greater than anything which can be translated into words. It is an idea which represents the integral of all the colours of which the prism shows us that white light is really composed.

It is quite easy by experiment to determine that the law which, as we have seen, Newton found at work in sunlight, is really equally operative in all white light, whether we get it from a lamp, or candle, or, indeed, from any white flame. We get what is called a *continuous spectrum*, that is, a spectrum in which there is light of every degree of refrangibility with no gaps or spaces.

The experiment indicated in the accompanying figure is one which anybody may make for himself or herself. We want a candle, and an ordinary lustre or prism placed as shown, at least twenty inches from the candle and on the same level. Then bearing in mind the fact that the beam is refracted or turned out of its course by the prism, we must place the eye,

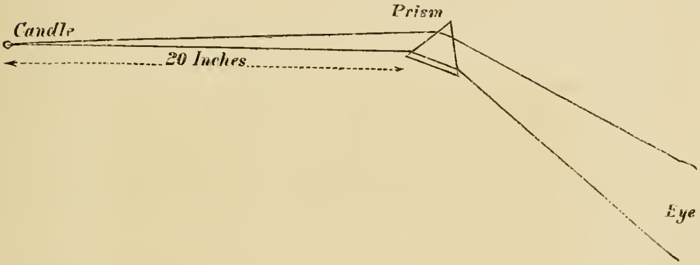

FIG 4.

not in the direct path of the light, but as indicated. We then get no longer an image of the candle, but a spectrum, presenting the series of colours in the following order proceeding from right to left :—

Violet. Indigo. Blue. Green. Yellow. Orange. Red.

This we may show graphically by representing these colours by their initials :—

V I B G Y O R

using open letters to show that it is a case of giving out of light.

This experiment will enable us to see the importance of a precaution which we owe to Newton. In his very first experiments he found that when we wish to get the best possible effect out of a prism, we must so arrange it that the particular ray which we wish to observe, whether the yellow, the blue, the green, or any other, leaves that prism at exactly the same angle as the incident compound ray falls on it. This angle is termed the *angle of minimum deviation.*

So much, then, for what we have learnt from Newton touching sunlight. He was far from grappling with the grand problem of solar chemistry; but he saw at once that it was impossible to imagine the sun to be a cool body, or to assume for it a different origin from our own planet. In his *Optics* he asks, "Are not the sun and stars great earths vehemently hot?"

When Newton made his classical experiments, he used, as we have already stated, a beam of light coming through a circular hole in a shutter; but he was soon able to prove to himself that the circular aperture was not the best thing he could use, because in the spectrum he had a circle of colour representing every ray into which the light could be broken up, and all these circles overlapped and produced a mixed and very impure spectrum. But although he was conscious of the defects of his method, he did not take the best steps to remedy them. We must never forget that in his time the art of making glass had made but a small advance. It was not until the lapse of 140 years that another step forward was made by Dr. Wollaston, who first employed a very narrow linear slit, an arrangement which gives a very pure spectrum, and is, therefore, adopted in all modern instruments.

In consequence of this improvement, Wollaston was enabled to make a discovery of the highest importance, the first real step, in fact, taken in Solar Chemistry.

No sooner had he introduced the fine slit for the examination of the solar spectrum than he observed that the spectrum was not absolutely continuous, as Newton had stated it to be, but that it was interrupted by a series of dark spaces.

This fundamental observation must here be given in Wollaston's own words, extracted from his communication to the Royal Society:[1]—

"The colours into which a beam of white light is separated by refraction appear to me to be neither seven, as they are usually seen in the rainbow, nor reducible by any means (that I can find) to three, as some persons have conceived; but that, by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen with a degree of distinctness that, I believe, has not been described nor observed before.

"If a beam of daylight be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of ten or twelve feet, through a prism of flint glass free from veins, held near the eye, the beam is seen to be separated into the four following colours only:—Red, yellowish-green, blue, and violet, in the proportions represented in Fig. 5.

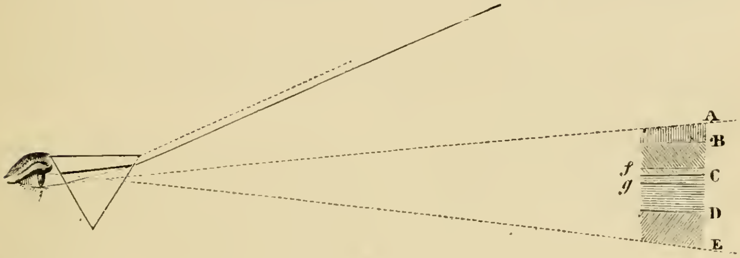

FIG. 5.—Copy of Wollaston's Diagram.

"The line A that bounds the red side of the spectrum is somewhat confused, which seemed in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet; but C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark spaces, *f* and *g*, either of which in an imperfect experiment might be mistaken for the boundary of these colours."

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[1] *Phil. Trans.* 1802, part i. p. 378.

Here, then, is an alteration in the solar spectrum with a vengeance.

But this is only one part of the story. Wollaston is the true founder of spectrum analysis; for, entirely unaware of the tremendous importance of the work he was doing, he showed that we may get artificial light sources, the spectra of which are *discontinuous*, but for a very different reason, as we now know.

He writes:—

> "By candle-light a different set of appearances may be distinguished. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum may be seen divided into five images at a distance from each other. The first is broad red, terminated by a bright line of yellow, the second and third are both green, the fourth and fifth are blue, the last of which appears to correspond with the division of blue and violet in the solar spectrum and the line D of Fig. 5.
>
> "When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images, but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain."

We shall see the great importance of these observations in the sequel. With Wollaston we quit the introductory stage of our subject.

CHAPTER II.

FRAUNHOFER, 1814.

In 1814 Fraunhofer extended Wollaston's work in both directions, and he further introduced another improvement by examining the emergent beam from the prisms directly by means of a telescope, instead of allowing it to fall on a screen, and then examining it with the naked eye, as Wollaston had done. In this way he got a magnified view of the spectrum. In order to insure the utmost purity in some of his experiments the slit was placed at a distance of 92 feet from the prism.

As may easily be imagined, by the aid thus afforded, he was enabled to observe a much greater number of lines in the solar spectrum than had been seen by Wollaston. He constructed a map, of which Fig. 6 is a much reduced copy, in which he recorded the positions of no less than 576 lines, distinguishing the most prominent ones by letters of the alphabet. From this time these dark lines have been called Fraunhofer lines.

He recorded also that the intensity of the light varied greatly in different parts of the spectrum, being greatest in the yellow and falling away rapidly on each side, as shown by the curved line in the figure.

Fraunhofer's work, in this direction, the importance of which

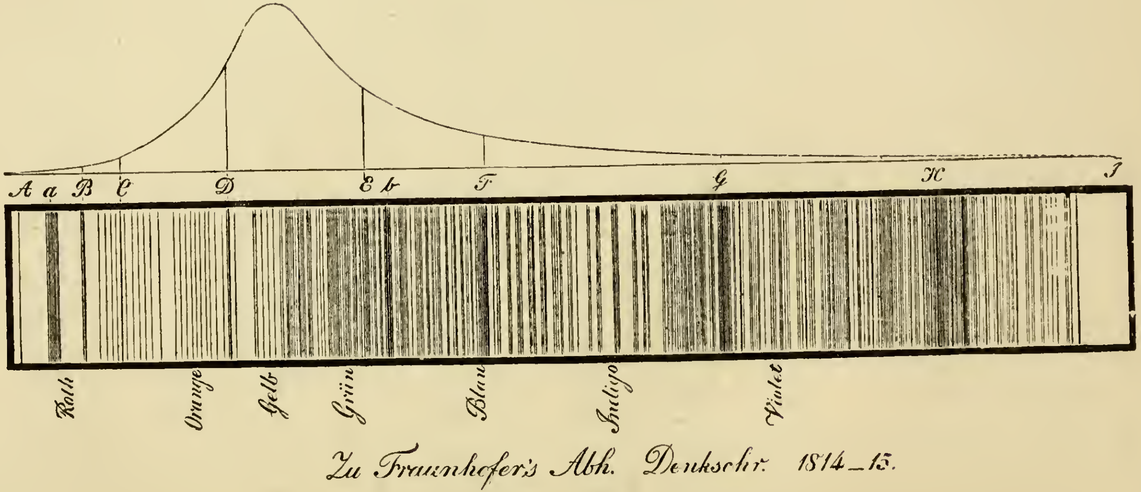

FIG. 6.—Reduced copy of Fraunhofer's map.

cannot be over-estimated, will be gathered from the following extract from his communication to the Munich Academy:—[1]

"Into a dark room, and through a vertical aperture in the window-shutter, about 15″ broad and 36″ high, I introduced the rays of the sun upon a prism of flint glass placed upon the theodolite; this instrument was 24 feet from the window, and the angle of the prism was nearly 60°. The prism was placed before the object glass of the telescope, so that the angles of incidence and emergence were equal. In looking at this spectrum for the bright line which I had found in the spectrum of artificial light, I discovered, instead of this line, an infinite number of vertical lines of different thicknesses. These lines are darker than the rest of the spectrum, and some of them appear entirely black. When the prism was turned so that the angle of incidence increased, these lines disappeared, and the same thing happened when the angle was diminished. If the telescope was considerably shortened, these lines reappeared at a greater angle of incidence; and at a smaller angle

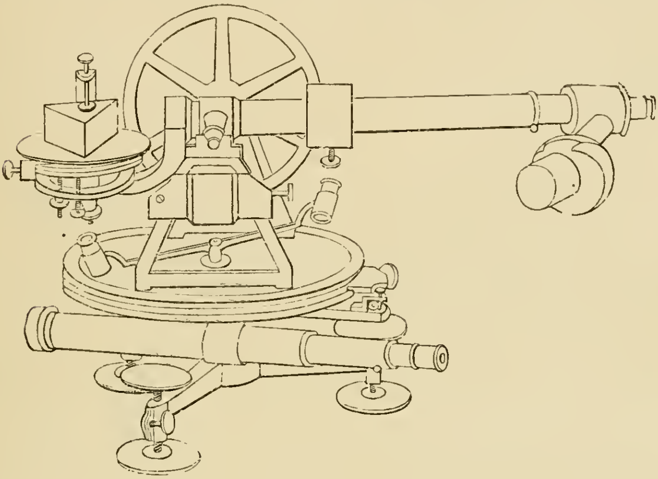

FIG. 7.—Fraunhofer's theodolite spectroscope.

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[1] *Denkschriften de K. Acad. der Wissenschaften zu München*, 1814-15, Band v. pp. 193-226. Translated in *Edinburgh Philosophical Journal*, vol. ix. p. 296, 1823, and vol. x. p. 26, 1824.

of incidence the eye-glass required to be pulled much further out in order to perceive the lines. If the eye-glass had the position proper for seeing distinctly the lines in the red space, it was necessary to push it in to see the lines in the violet space. If the aperture by which the rays entered was enlarged, the finest lines were not easily seen, and they disappeared entirely when it was about 40″.

"If it exceeded a minute the largest lines could scarcely be seen. The distances of these lines and their relative proportions suffered no change, either by changing the aperture in the shutter, or varying the distance of the theodolite. The refracting medium of which the prism is made, and the size of its angle did not prevent the lines from being always seen. They only became stronger or weaker, and were consequently more or less easily distinguished in proportion to the size of the spectrum. The proportion even of these lines to one another appeared to be the same for all refracting substances; so that one line is found only in the blue, another only in the red, and hence it is easy to recognise those which we are observing. The spectrum formed by the ordinary and extraordinary pencils of calcareous spar, exhibited the same lines. The strongest lines do not bound the different colours of the spectrum, for the same colour is almost always found on both sides of a line, and the transition from one colour to another is scarcely sensible.

"Fig. 6 shows the spectrum with the lines such as they are actually observed. It is, however, impossible to express on this scale all the lines and the modifications of their size. At the point A the red nearly terminates and the violet at I. On either side we cannot define with certainty the limits of these colours, which, however, appear more distinctly in the red than in the violet. If the light of an illuminated cloud falls through the aperture on the prism, the spectrum appears to be bounded on one side between G and H, and on the other at B; the light of the sun, too, of great intensity, and reflected by a heliostat, lengthens the spectrum almost one-half. In order, however, to observe this great elongation, the light between C and G must not reach the eye, because the impression of that which comes from the extremities of the spectrum is so weak as to be extinguished by that of the middle of the spectrum. At A we observe distinctly a well-defined line. This, however, is not the boundary of the red, which still extends beyond it. At *a* there is a mass of lines forming together a band darker than the adjacent parts. The line at B is very distinct, and of a considerable thickness. From B to C may be reckoned nine very delicate and well-defined lines. The line at C is broad and black like D. Between C and D are found nearly thirty very fine lines, which, however, with the exception of two, cannot be perceived but with a high magnifying power and with prisms of great dispersion; they are besides well defined. The same is the case with the lines between B and C. The line D consists of two strong lines separated by a bright one. Between D and E we recognise eighty-four lines of different sizes; that at E consists of several lines, of which the middle one is the strongest. From E to *b* there are nearly twenty-four lines; at *b* there are three very strong ones, two of which are separated by a fine and clear line; they are among the strongest in the spectrum. The space *b*—F contains nearly fifty-two lines, of which F is very strong. Between F and G there are about 185 lines of different sizes; at G many lines are accumulated, several of which are remarkable for their size. From G to H there are nearly 190 different lines. The bands at H are of a very singular nature; they are both nearly equal, and are formed of several lines, in the middle of which there is one very strong and deep. From H to I they likewise occur in great numbers. Hence it follows that in the space BH there are 574 lines, the strongest of which are shown in the figure. The relative distances of the strongest lines were measured with the theodolite, and placed in the figure from observation. The faintest lines only were inserted from estimation by the eye."

Here then we have the first substantial contribution to a map of the newly discovered lines in the spectrum of the sun.

Fraunhofer next explained why the lines are not well marked, and why they disappear if the aperture of the slit becomes too large. If the aperture is not such that the light which passes through it cannot be regarded as a single ray, or if the angle of the width of the aperture is greater than that of the width of the line, then the image of the same line will be projected several times parallel to itself, and will consequently become indistinct, and disappear when the aperture is too great.

He was not content with this work. He wanted to know something of the origin of the lines, and he soon came to a conclusion on this point. It occurred to him that they might possibly be attributed to some illusion caused by the narrow aperture through which the light was admitted. We shall see subsequently that the slit has something to do with the *forms* of these dark spaces, but with their simple existence as spaces it has nothing to do, the mere shape of the lines being quite a trivial matter. To test this he had recourse to a very ingenious method, which is best described in his own words:—[1]

> "In observing the great quantity of lines in the solar spectrum we might be led to believe that the inflection of light at the narrow aperture in the window-shutter had some connection with them, though the experiments described do not give the least proof of this, and indeed establish the contrary opinion. In order to put this beyond a doubt, and also to make some other observations, I varied the experiments in the following manner: if we make the sun's rays pass through a small round aperture in the window-shutter, nearly 15″ in diameter, and cause it to fall on a prism placed before the telescope of the theodolite, it is obvious that the spectrum seen by the telescope can only have a very small width, and consequently will form only a line. In a line, however, almost no breadth it is impossible to see the fine and delicate lines which traverse it; and, on that account, the fixed lines are not seen in a spectrum of this kind. In order, however, to see all the lines in this spectrum, it is necessary only to widen it by an object-glass, without altering its length. I obtained this effect by placing against the object-glass a glass having one of its faces perfectly plane, and the other ground into the segment of a cylinder of a very great diameter. The axis of the cylinder was exactly parallel to the base of the prism. . . ."

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[1] *Edin. Phil. Journal*, vol. x. pp. 37 and 38.

This experiment quite convinced Fraunhofer that interference would not account for the dark lines, and he was soon able to announce that—

> "Various experiments and changes to which I have submitted these lines convince me that *they have their origin in the nature of the light of the sun,* and that they cannot be attributed to illusion, to aberration, or any other secondary cause."[1]

He then states that he applied this method to the examination of the spectra of Venus and certain stars, "without allowing the light to fall on a small aperture." Here we have Fraunhofer introducing a method of observation of stellar spectra that is still the one employed in our own times.

He was rewarded by being enabled to make the following observations on the spectrum of Venus :—[2]

> "In the spectrum formed by this light, I found the same lines such as they appeared in the light of the sun. That of Venus, however, having little intensity compared with that of the sun reflected from a mirror; the brightness of the violet and the exterior red rays is very feeble. On this account we perceive even the strongest lines in these two colours with some difficulty; but in the other colours they are easily distinguished. I have seen the lines D, E, *b*, F (Fig. 6) very well terminated; and I have recognised that those in *b* are formed of two, namely, a fine and a strong line. The weakness of the light, however, prevented me from seeing that the strongest of these two lines consisted of two; and, for the same reason, the other finer lines could not be distinguished. By an approximate measure of the lines D E and E F, I am convinced that the light of Venus is, in this respect, of the same nature as that of the sun."

Then, coming to the stars :—

> "With the same apparatus I have also made several observations on some of the brightest fixed stars. As their light was much

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[1] *Edin. Phil. Journal*, vol. x. p. 38.
[2] *Ibid.* vol. ix. p. 298.

> fainter than that of Venus, the brightness of their spectra was consequently still less. I have nevertheless seen, without any illusion in the spectrum of the light of Sirius, three large lines, which apparently have no resemblance with those of the sun's light. One of them is in the green, and two in the blue, space. Lines are also seen in the spectrum of other fixed stars of the first magnitude."[1]

No wonder that a man who had thus brought sun, planet, and star within the grip of a new instrument should not rest content with these observations.

He next investigated at considerable length the spectra of artificial light sources. In an early part of his paper[2] he states that, on examining the spectra of artificial flames, he found that flames such as that of a lamp or candle, and, indeed,

> "In general, the light produced by the flame of a fire, exhibit between the red and yellow of the spectrum a clear and well-marked line, which occupies the same place in all the spectra. This line will become more important in the sequel, and it was one of great utility to me. It appears to be formed by rays which are not decomposed by the prism, and which consequently are homogeneous. In the green space we perceive a similar line, but it is weaker and less distinct, so that it is often very difficult to find."

Returning to this subject later on he notes[3] that in transmitting the light of a lamp through the same aperture employed for the examination of the solar spectrum, he observed a line corresponding exactly in position with the D line in the solar spectrum; nay, both the lines were double!

> "In making the light of a lamp fall through a narrow aperture, from 15″ to 30″ wide, upon a prism of great dispersion, placed before the telescope, we perceive that the red line of this spectrum

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[1] *Edin. Phil. Journal*, vol. x. pp. 38, 39.

[2] *Ibid.* vol. ix. p. 291.

[3] *Ibid.* p. 298.

is formed by two very delicate bright lines, similar in size and in distance to the two dark lines D, Fig. 6. Whether the aperture through which the light of the lamp passes is wide or narrow, if we cover the point of the flame and the lower blue extremity of it, the red line appears less clear, and is more difficult to be distinguished. Hence it appears that this line derives its origin principally from the light of the two extremities of the flame, particularly the inferior one."[1]

But Fraunhofer did not confine himself to flames; he investigated the light produced by electricity:—[2]

"The electric light is, in relation to the lines of the spectrum, very different from the light of the sun and of a lamp. In this spectrum we meet with several lines, partly very clear, and one of which, in the green space, seems very brilliant, compared with the other parts of the spectrum. Another line, which is not quite so bright, is in the orange, and appears to be of the same colour as that in the spectrum of the light of a lamp; but, in measuring its angle of refraction, I find that its light is much more strongly refracted, and nearly as much as the yellow rays of the light of a lamp. Towards the extremity of the spectrum we perceive in the red a line of very little brightness; yet its light has the same refrangibility as that of the clear line of the light of a lamp. In the rest of the spectrum we may still easily distinguish other four lines sufficiently bright."[3]

To sum up Fraunhofer's first work, we see that he gave us a greatly improved method of observation, and then, as a result of the improved method, the first map of the dark lines in the solar spectrum. Next, by the introduction of the cylindrical lens, he extended our range of observation to the planets and stars, and on applying this method discovered that the spectra of the planets are essentially the same as that of the sun, whilst the stars have independent spectra. Coming to artificial flames he found that their spectra are characterised by the presence of bright lines, and that one, at least, of these lines is exactly matched in width, duplexity, and position by a similar double line in the solar spectrum. Finally we have his observation that the spectral phenomena presented by a candle flame and an electric spark are localised in special regions.

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[1] *Edin. Phil. Journal.* vol. x. p. 39.

[2] *Ibid.* p. 39.

[3] "In order to obtain a continuous electrical light, I brought to within half an inch of each other, two conductors, and I united them by a very fine glass thread. One of the two was connected with an electrical machine, and the other communicated with the ground. In this manner the light appeared to pass continuously along the glass fibre, which consequently formed a fine and brilliant line of light."—*Ibid.* (*note*).

We have not even yet exhausted the contributions of this remarkable man to our subject. He brought the study of the diffraction of light, as well as its refraction, to a point at which it could be utilised for the furtherance of solar research. Discarding the old methods of observation, or rather improving them, he replaced the edge and aperture employed up to that time by a fine system of wires, the intervals between which provided him with parallel and equi-distant apertures in great number.[1]

Observing diffraction phenomena in this manner he was delighted to see what had formerly been observed as coloured fringes merely, developed into beautiful spectra.

> " On aperçoit dans les spectres produits par le fils du réseau les meme raies, les meme bandes, que j'ai découverte dans les spectres produits par la lumière solaire au moyen de bons prismes."

In the parallel wires of Fraunhofer we have the forerunner of the "grating," or "*réseau*," the utilisation of which we shall see in the sequel.

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[1] For Fraunhofer's paper, "Neue Modifikation des Lichtes," see *Denkschriften der K. Akademie zu München*, 1821-1822; *Classe der Mathematik und Naturwissenschaften*, pp. 1-76; also Gilbert's *Annalen*, 1823, and Schumacher's *Tracts*, 1823, p. 46.

CHAPTER III.

THE FIRST GLIMPSES OF "BRIGHT LINE SPECTRA."

The phenomena first observed by Wollaston and Fraunhofer must now be referred to in more detail; it will be seen in the sequel that they lie at the root of all the subsequent work. In the first chapter I described an experiment by which it is possible to convince ourselves, with the aid of no other apparatus than a glass lustre-drop and a candle, that white light is a composite thing. It will now be well to see whether we can familiarise ourselves with the developments of the subject we owe to the two men I have named, by means of very simple experiments.

Now we first learnt from Wollaston that we must use a slit, or crevice as he called it. Let us then cut a fine slit, say one-twentieth of an inch broad and half an inch long, carefully out of a piece of tin-foil, and gum the tin-foil with this "crevice" in it on to a plate of glass. Then we can make also in the same manner a narrow circular slit about half an inch in diameter and vary the previous experiment in this way. Let us place the straight slit where the candle was in the first experiment, and then place the candle close behind it, so that, so to speak, only a slice of the candle light will pass through the slit to the eye placed as before. It is immaterial on which side we place the base of the prism provided the light enters and leaves the refracting surfaces at equal angles. When the line-slit, say twenty inches off, is observed with the refracting edge of the prism parallel to its length, a very brilliant spectrum of the candle is obtained, even though it be wanting in definition. This latter can be improved if a narrower slit is employed; for in spectra almost all impurity comes from the overlapping of images. The operations of nature are so delicate, that it seems as if a pure colour, such as a pure blue or a pure red, will for ever remain an abstraction; for, however great the dispersion, the adjacent rays will remain commingled, and commingled rays define a compound colour.

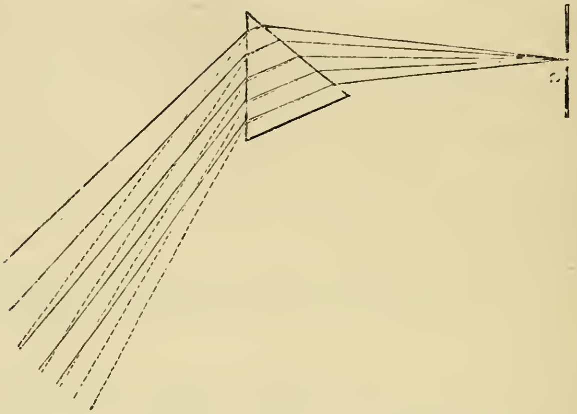

FIG. 8.—Showing arrangement of slit and prism (c slit).

Instead of reducing the width of the slit, if it be not connected with the prism by means of a tube as it may conveniently be, the slit can be removed further from the prism. In this way we get apparently a narrower slit without any reduction in the quantity of light which passes through it to the eye. A gas-flame or a candle placed in front of this slit is all that is necessary to produce that great purification of the spectrum first effected by Wollaston.

We may now go a step further. A spectacle lens may be inserted in the beam as it leaves the prism. Here we are approximately brought face to face with the improvement introduced by Fraunhofer. This lens will throw an image of the spectrum on a screen or a piece of paper placed at a distance from it determined by the length of the focus of the lens.

But Fraunhofer did more than this, he used a small telescope, no screen was necessary, and the spectrum appeared to the eye greatly magnified by the eyepiece. Similarly we may use an opera-glass or a small telescope instead of the simple spectacle lens.

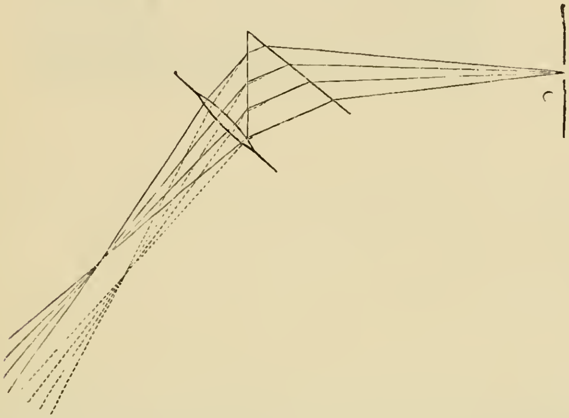

Fig. 9.—Introduction of a lens to produce an image (c slit).

While on this part of our subject we may as well say one word on another addition to the prism and telescope as used by Fraunhofer, made by Mr. Simms in 1839. This has given us the spectroscope as we now know it.

Fraunhofer, as we have seen, in order to get good definition with his little telescope, required the slit to be some considerable distance (as much as 642 feet in one set of experiments) in front of the prism; not only did this arrangement require much space, but the various rays from the slit entered the prism at different angles, and in consequence the purity of the spectrum was considerably impaired. It occurred to Mr. Simms,[1] the celebrated optician, and independently to Professor Swan,[2] that that difficulty and the difficulty of space would be obviated if the incident pencil was converted into a parallel beam before it entered the prism. This was effected by the introduction of a second lens, called a "*collimating*" lens, between the slit and the prism. This last improvement introduced complete compactness, and as I said before, made the instrument practically such as we now employ.

Whilst describing the various steps which have led to the perfection of the modern spectroscope, it is well to refer in this place to an arrangement suggested by Janssen. The prism employed in this arrangement differs from the simple one very much as the achromatic telescope differs from the non-achromatic one. The object-glass of a telescope, as now constructed, consists of two lenses made of different kinds of glass. Of course, we have dispersion and deviation (terms already explained) at work in both these kinds of glass, but the lenses are so arranged, and their curves are so chosen, that, as a total result, the deviation is kept while the dispersion is eliminated, so that, in the telescope, we have a nearly white image of everything which gives us ordinary light. So also in the spectroscope we have an opportunity of varying the deviation and the dispersion. By a converse arrangement we can keep the dispersion while we lose the deviation; in other words, we have what is called a *direct-vision* spectroscope. If we take one prism composed of glass which possesses considerable refractive power, and two prisms of another kind which does not refract so strongly, and arrange them with their bases the opposite way, the deviation caused by the one prism in the one direction, will be neutralised by the deviation of the two prisms in the opposite direction; whilst the dispersion by the two prisms exceeds that which is caused by the one prism in the opposite direction; the latter dispersion, therefore, will neutralise a portion only of the dispersion due to the two prisms. The final result is that there is an outstanding dispersion after the deviation has been neutralised; so that when we want to examine the spectrum of an object we no longer have to look at it at an angle, but by this arrangement we have an opportunity of seeing the spectrum of an object by looking straight at the source of light.

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[1] Memoirs R. A. S. 1839, vol. xi. pp. 168, 169. Mr. Simms in this paper (describing the measurement of the refractive index of the optical glass prepared by the late Dr. Ritchie) states: "the only novelty of any consequence in this instrument (the spectroscope employed) is the substitution of a collimator in place of a slit in a window shutter."

[2] *Trans. Roy. Soc.* Edin. 1847 and 1856.

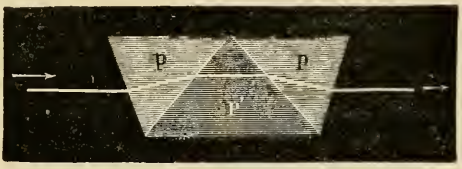

FIG. 10.—Direct-vision prism with three prisms, showing path of ray.

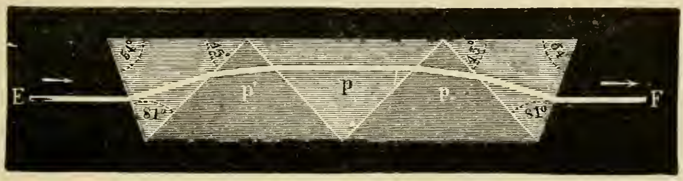

FIG. 11.—Direct-vision prism with five prisms.

If we now pass from the improved methods already introduced by Wollaston and Fraunhofer to the phenomena they observed whether in solar light, or the terrestrial light sources which they examined, we find revelations of the first order of importance.

The candle which was used in our first experiment (page 9) was a source of white light; there are, however, as Wollaston first showed us, many artificial flames which are coloured, and if their light is analysed in the same way as the light of the candle, a perfectly new set of phenomena present themselves. Wollaston found that the spectrum of the blue part at the base of a candle-flame was not continuous, but was accumulated, as it were, here and there, into patches of greater intensity than elsewhere. Especially he observed a very bright patch in the yellow. Fraunhofer as we have seen carried the work further.

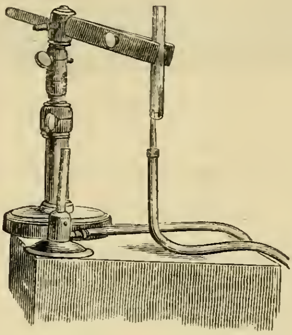

Fig. 12.—Improvised Bunsen burner.

In dealing with these results experimentally we need not confine our inquiries to the exact grooves in which Wollaston's and Fraunhofer's work ran, and it cannot be too generally known that an expenditure of a few shillings is all that is required to enable us to study the results for ourselves. This money should be expended in buying two little brass or glass tubes, one-tenth and half inch in diameter and five inches long, a little glass tubing of very small bore, a few inches of platinum wire, a small quantity of red and green fire and chloride of lithium, and india-rubber tubing to convey gas from an ordinary burner to a table. Of the two tubes a Bunsen burner can be easily constructed. This is an apparatus for burning a mixture of ordinary gas and air; the gas being supplied through the smaller tube inserted into the lower end of the larger one in which the mixture takes place as shown in fig. 12. The gas should be lit by holding a match some three inches above the upper orifice of the wide tube. One end of the platinum wire may be fused into the piece of glass tubing, and the other twisted into a loop fine enough to hold some common salt in the flame. A piece of coke or charcoal soaked in salt and water will do almost as well. This tube may be supported by a piece of wood, after the fashion of Fig. 13. The Bunsen burner will give us a very hot bluish flame into which the loop of platinum containing common salt (sodic chloride) may be inserted, as shown in the accompanying woodcut. We shall get a brilliant yellow flame which is worth notice on its own account.

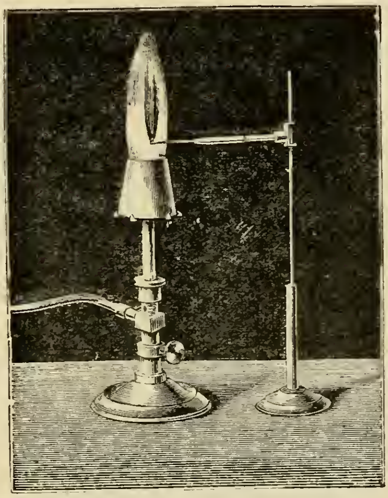

FIG. 13.—Method of inserting platinum wire and salt into flame.

Instead of this improvised Bunsen burner we may use—as the old observers before the introduction of gas were compelled to use—a spirit-lamp; but such a lamp is not the best instrument for such inquiries, because the temperature of its flame is too low to volatilise many substances which otherwise could be used to colour it. In these days of gas we are able to supersede the spirit-lamp by the Bunsen burner, the flame of which is hotter than that of the spirit-lamp.

Suppose now the flame to be impregnated with some substance which colours the flame a bright red. If such a flame is examined in the manner already indicated, we see at once why the flame is red, for the light is not dispersed, it behaves exactly as the red light did which Newton filched from his continuous spectrum of sun light and made to pass through a second prism. It contains no orange, yellow, green, blue, or violet rays, and therefore its spectrum instead of containing all these colours will lie wholly in the red, so that we should not represent the spectrum, using the graphical notation already introduced (p. 9), by

V I B G Y O R

as we did in the case of the candle, but simply by

R

If we use a substance which colours the flame yellow we shall get simply

Y

and similarly a substance with a green flame would give us

G

only.

So much for a first general statement: now let us go a little more closely into detail. Place the improvised Bunsen burner and the platinum wire with the salt on it in front of the straight slit and look at the slit through the prism; it will be found that there is only a yellow line visible. If the things have been nicely arranged the appearance of the spectrum will be so entirely changed that a beginner will be apt to fancy that something has gone wrong. Nothing has gone wrong however, we have simply passed from the spectrum of polychromatic to that of monochromatic light—from white light to coloured light; instead of

V I B G Y O R

we get light in the yellow at

Y

Next try the circular slit. Instead of a line we get a circle in the same part of the spectrum.

What we shall see in passing from the spectrum of the candle to the spectrum of the sodium vapour as seen by a straight or a circular slit is shown in the accompanying woodcut.

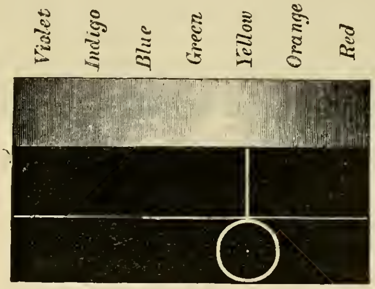

Fig. 14.—The spectra of continuous and discontinuous light-sources, the latter seen with a line and circular slit.

If we treat a salt of lithium in the same manner we shall see with the line slit a bright line in the red, with the circular slit we shall get a circle in the same place. A salt of thallium will, according to the slit we use, give a bright line or circle in the green, and if we examine a very complicated light source we shall arrive at the same result—a spectrum characterised by a large number of bright lines or circles, depending upon the shape of the slit, in different parts of the spectrum. We do not get the complete spectrum represented by

V I B G Y O R

but, in the case of sodium, only

Y

lithium, only

R

with a substance which gives us a complicated spectrum we see lines, as under, here and there; *the light in each of these cases assuming in the telescope the form of the slit,* and occupying different positions along the spectrum according to the light source. Here then we are in possession of the bright line phenomenon first so called by Fraunhofer.

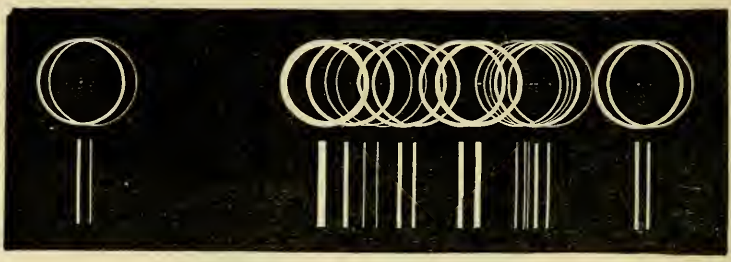

Fig. 15.—The spectrum of a complicated light-source as seen with a circular and a line slit.

Now why do we see lines? For this reason—

The light passing through the lenses forms an image of the slit because in this case the prism deviates the light without dispersing it, and we should see an image of the slit if we used the collimator and telescope alone without the prism.

Now apart from the difficulty of constructing fanciful forms of slits there is one very powerful reason that leads to the preference of the line above all other forms. So long as we are using a high dispersion and dealing only with bodies that give out light of very few colours, it will not make very much difference if we use, say, a circular slit—but when we come to substances that give very complex spectra, or when we use only a low dispersion, it is evident we shall get the result shown in the last figure. The different coloured circles will overlap each other and the whole spectrum will be in a state of confusion. For the sake of comparison the same spectrum is shown as observed with a line slit. When a line slit is used we can make it as narrow as we please, and so reduce the overlapping to a minimum and get a spectrum of the greatest purity.

After these statements the meaning of the term "line spectra" should not present any difficulty.

We may now then dismiss this part of our subject, in which Wollaston was the pioneer, with the remark that spectra of bright lines are produced by the giving out of light by light-sources which are coloured. In other words, the radiation of coloured light-sources when examined by the spectroscope gives us bright line spectra.

CHAPTER IV.

BREWSTER, HERSCHEL AND FORBES; BECQUEREL AND DRAPER.

1822—1843.

The work of Wollaston and Fraunhofer then, and the improvements in the methods of observation which they effected, taught us that the quality of the sun's light was very special and that the solar spectrum was a thing *per se.* Fraunhofer, as we have seen, was firmly of opinion that the special quality was imparted at the sun itself.

We learnt from both of them, moreover, that while sunlight, on the one hand, differed from ordinary white light in having a spectrum of dark lines; coloured light, on the other hand, differed from the same white light, in having a spectrum in which bright lines are seen.

It is in Sir David Brewster's work that we find the next important development.

Like his predecessors, he too made a map of the solar spectrum, and, like them too, he was the first to open out a branch of research on which our present knowledge of solar chemistry is based.

In 1822 he laid before the Royal Society of Edinburgh[1] the results of experiments he had performed, with the view of constructing a monochromatic lamp for microscopical purposes to prevent the coloured fringes produced by the imperfectly corrected lenses then used in the construction of high power object-glasses. He had observed that burning paper, linen, cotton, &c., gave flames in which the yellow rays predominated, and this effect was most marked in the flame of alcohol diluted with water. On examining this flame with the aid of a prism, he found that the great bulk of the light was located in the yellow, and though there were faint traces of green and blue, there was not a trace of red. So far, there is nothing particularly new. But he next proceeded to open up a new field of observation altogether. In the course of his experiments he examined *the action of various coloured bodies on the different parts of the spectrum.* He found not only that different coloured bodies *cut out* different rays from the spectrum, but that this action of the same body varied with its thickness and temperature.

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[1] *Edin. Phil. Trans.* vol. ix. 1823, p. 433 *et seq.*

Shortly after this paper appeared, Mr. J. W. F. Herschel,[1] in a letter to Sir David Brewster, described a similar series of experiments which he had made. In one of these he projected a spectrum obtained by a circular opening and a prism on a sheet of white paper, and examined it through a glass which cut out all but the red rays. He found that the light was perfectly homogeneous, and produced a well-defined circular image of the aperture.

Here then was an instance of light coloured red, not because it was originally tuned to vibrate at that one rate only, for the light was originally complete, and gave the continuous spectrum,

V I B G Y O R

but because in its passage through the glass the violet, indigo, blue, green, yellow, and orange were cut out and stopped, thus,

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[1] *Edin. Phil. Trans.* vol. ix. 1823, p. 445 *et seq.*

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and by the defect of these rays the light became red.

To record his observations Herschel adopted a device which has since come into very general use. He represented the prismatic spectrum by a horizontal line, indicating the various colours by their initial letters, and then at all points where light was transmitted he erected perpendiculars of lengths proportionate to the intensity of the light observed; connecting the extremities of all these lines he obtained a curve which he called the type of that particular medium.

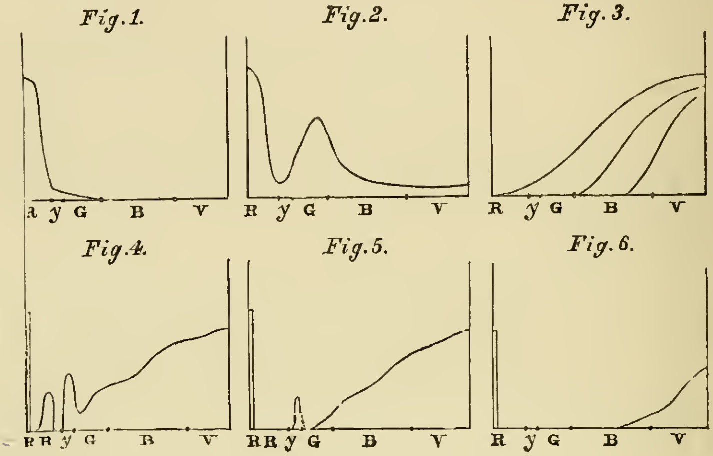

FIG. 16.—Copies of Herschel's Diagrams.

The accompanying figures which are copies of some of those given by Herschel, show the sort of curves obtained.

No. 1 represents the effect of ruby-red glass or port wine on the spectrum. Nearly all the light transmitted is in the red. No. 2 shows the effect of a particular green glass with a tendency to redness. 3 shows the action of a solution of ammonio-carbonate of copper in various thicknesses, the absorption commencing in the red, and gradually extending over everything but the violet. 4, 5, and 6 represent the effect produced by various thicknesses of a blue glass with a tinge of purple, 4 showing the action of the least thickness, and 6 that of the greatest.

It will be seen from these figures that Herschel observed that this stoppage of light affected not only large reaches of the spectrum, but as shown in Fig. 16 (4), *it was sometimes so closely localised as to produce sharp lines.* He was the first to point out this fact clearly. These phenomena are produced by the *absorption* of light by the various bodies interposed in the path of the beam on its way to the eye. We shall have to dwell on this effect at considerable length in the sequel.

Such experiments as these explain the phenomena of sunset colours, which deserve notice here because, as it will be seen later on, the work done by our atmosphere on the sunlight requires to be completely studied before we can get any certain knowledge of the light actually emitted by the sun.

One form of the aqueous vapour in our atmosphere exerts a powerful obliterating action on the solar light, commencing at the blue end, and gradually encroaching on other parts of the spectrum, thus:—

V I B G Y O R No absorption.
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R
V I B G Y O R

Showing the effect of increasing absorption.

so that when the sun is near the horizon, and shines through the greatest thickness of our atmosphere, his light appears red because all the other colours are to a great extent absorbed.

The next work of Brewster's carries us much farther in the same direction. He announced to the Royal Society of Edinburgh,[1] in 1833, that he had examined the lines of the solar spectrum with various optical appliances, and had delineated them on a scale four times greater than that employed in the beautiful map of Fraunhofer. Some portions also, which were more particularly studied, had been drawn on a scale twelve times greater. Fraunhofer's spectrum was fifteen and a half inches long; a map on the largest scale employed by Brewster would be seventeen feet long.

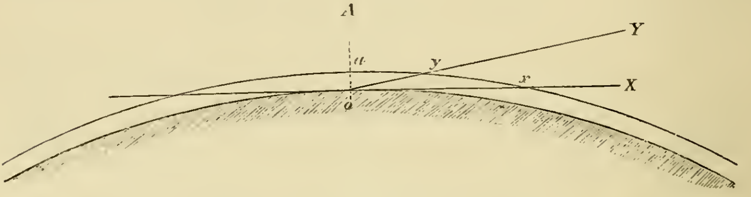

FIG. 17.—Diagram showing the increasing thickness of air through which the light of the sun has to pass as it descends towards the horizon. When the sun is in the zenith, A, its light will have to pass through a thickness of atmosphere represented by $a\ o$. When near the horizon, this distance will be increased to $y\ o$, and when actually on the horizon to $x\ o$.

But this was not all. In this same paper Sir David Brewster described

> "*a remarkable series of dark lines and bands, which made their appearance in the spectrum when nitrous acid gas was interposed between the prism and the source of light,* whether that were the sun or a burning lamp."

In these experiments we get the first adequate glimpse of those wonderful phenomena produced when the light on its way to the eye is made to pass among the molecules of more or less transparent substances. The phenomena were so definite that the experiments did not go on very long before the conviction was forced on Brewster that a new method of chemical analysis was possible by these means, and that such a *spectrum analysis* was destined to become a chemical agent of great value. In his paper he says, "the first and principal object of my inquiries" was "the discovery of a general principle of chemical analysis, in which simple or compound bodies might be characterised by their action on definite parts of the spectrum;" and he showed how he had already found it possible, by means of the variety and constancy of the effects thus observed, to distinguish the coloured juices of plants, solutions of salts, minerals, &c., "by merely looking through them at a well-formed spectrum."

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[1] *Edin. Phil. Trans.* vol. xii. 1834, p. 519 *et seq.*

These investigations of Brewster and Herschel can be easily and cheaply imitated by a few very simple additions to th improvised spectroscope described in Chapter III. To study the absorption of coloured glasses, or other solids, it is only necessary to interpose them between the candle and the slit, and to observe the spectrum as before.

The observation of a great many substances, in this manner, has intensified the interesting fact first revealed by Brewster, that while some bodies absorb equally all the prismatic rays, and so give rise to a general darkening of the spectrum, other bodies act locally, their absorption being confined to special regions. *So that we have general and selective absorption just as we have general radiation, giving a continuous spectrum, and selective radiation, giving lines here and there.*

To observe the effect of general absorption it is only necessary to employ a piece of smoked, or still better, neutral tinted glass, the only effect of which will be to deaden the spectrum along its whole length.

A piece of the ruby-red glass commonly used for photographic purposes will cut off nearly all but the red light, affording an instance of selective absorption, while certain kinds of blue glass will cut off all but the blue. By introducing both the pieces into the beam the spectrum is entirely obliterated.

When liquids have to be examined it is necessary to have some sort of transparent cell to hold them. It will not be necessary to buy such an apparatus; two squares of glass with a piece of india-rubber tubing between them bent thus U, the glasses being kept in contact with the tubing by two india-rubber bands, form a cell which is wonderfully tight, and will serve our purpose excellently. This, with the inclosed liquid, must be placed in front of the slit like the coloured glasses.

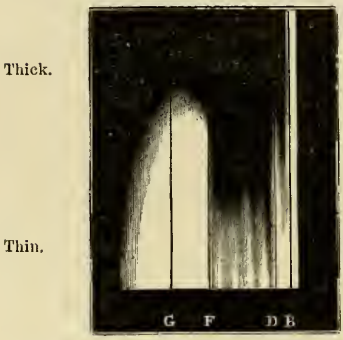

Fig. 18.—Absorption of sunlight by various thicknesses of a solution of the salts of chromium (Gladstone).

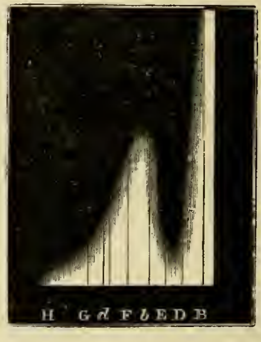

Fig. 19.—Absorption of sunlight by various thicknesses of a solution of potassic permanganate (Gladstone).

A little potassic permanganate, or Condy's fluid in water, thus used as a light-filter will produce a deep band in the yellow part of the spectrum and the adjacent regions of the orange and green. Solutions of blood or magenta will give also very definite indications of absorption. In the former case two broad bands of absorption in the green and yellow, and in the latter one in the green, will be seen.

To observe the effect of increasing thickness of the medium wedge-shaped cells may be employed, and it will then be found that as the thickness increases the absorption lines broaden out and new regions of absorption appear.[1] But we are not limited to solids and liquids. Gases and vapours can be made to yield absorption spectra. Put a few grains of iodine in a dry flask and place it in front of the slit; then warm the flask gently with a spirit-lamp. The iodine will soon volatilise and fill the flask with a violet vapour, giving rise to a beautifully regular fluted spectrum as shown in Fig. 21.

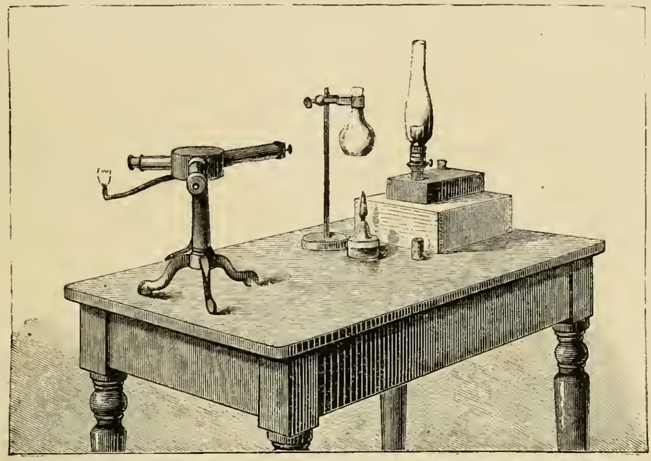

FIG. 20.—Method of studying the absorption of iodine vapour.

When the gas or vapour is not distinctly coloured it is necessary to pass the light through a great thickness of it. The manner in which this is effected is shown in Fig. 22. The gas is contained in a large glass tube closed at each end by a flat glass plate. At one end of the tube is placed a lamp, and at the other the spectroscope.

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[1] If we have one of those handy little pocket spectroscopes, which now, I am glad to think, are becoming common, the absorption of the light of a candle by the blood in the lobe of a friend's ear, or in the interval between two closed fingers, can be well seen by placing it between the slit and the light.

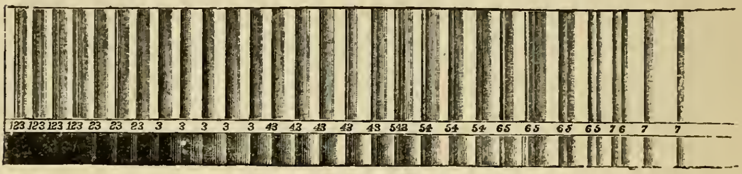

FIG. 21.—Absorption spectrum of iodine (Thalén).

We are now, then, in possession of the phenomena first observed by Brewster and Herschel, and of an easy means of producing them for our own satisfaction. We must now pass on to their applications.

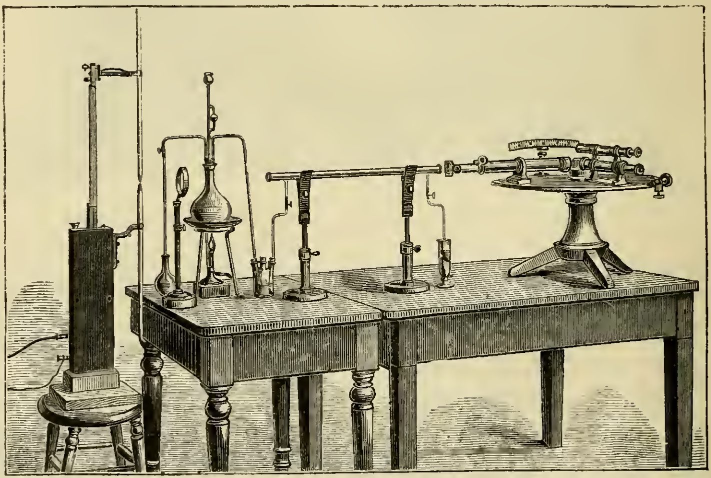

FIG. 22.—Method of observing the absorption of a great thickness of vapour.

Brewster at once applied this new knowledge to the sun.

He first of all endorsed Fraunhofer's view that the dark lines were truly solar in their origin; and more than this, he could now explain how the lines came about, for if a tube of nitrous oxide gas gave us lines identical in character with the solar lines as a result of its absorptive properties, what more natural than to suppose that the lines were produced by absorption in the atmosphere of the sun?

He went further. His observations led him to the conclusion that many of the lines produced by the absorption of the gas in question were identical in their position along the spectrum with several of the Fraunhofer lines themselves; and he felt himself justified in announcing, on the strength of these coincidences, the discovery that there was nitrous acid gas in the atmosphere of the sun. This is most interesting, for here we have the first chemical touch in solar inquiry.

Brewster went on to make another important observation:—

> "When the sun descends towards the horizon and shines through a rapidly increasing depth of air, certain lines which before were little, if at all, visible, become black and well defined, and dark bands appear even in what were formerly the most luminous parts of the spectrum."

He states that this effect was observed both at sunrise and sunset, and that it could not be due to any general obscuration caused by the increasing darkness at sunset, because it was not necessary that the sun should be very low on the horizon in order to produce these effects, but that they were visible while the lines H and K in the violet were still distinct, and therefore before any considerable darkening occurred, which would have obscured this end of the spectrum first. He therefore announced the discovery that the variable bands were "produced by the absorptive effect of the earth's atmosphere,"[1] and from the want of variation among the other lines he saw an indication—

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[1] *Loc. cit.* p. 528.

> "That the apparent body of the sun is not a flame in the ordinary sense of the word, but a solid body or coating raised by intense heat to a state of brilliant incandescence."[1]

It will be seen, then, that the study of the sun was now (1833) in full swing. We had at length, after waiting some centuries, a method of observing a spectrum; we had, further, the fact that there were dark lines in the solar spectrum; that coloured flames gave us bright lines; that certain substances stopped some of the light which passed through them, thus producing dark lines. Hence that the solar lines might be produced in the same way.

We next come to Forbes, whose manner of work was interesting, though it led him to a conclusion which we now know to be erroneous.

We have seen that both Fraunhofer and Brewster were firmly of opinion that the dark lines were produced by *some action at the sun.* Brewster was more definite, and said *some absorption at the sun.*

The next point investigated was this. It seemed obvious that if Brewster's view were true there should be a difference between the spectrum of the sun's centre and the sun's limb or edge, because the light passing to the eye from the latter would have to traverse a greater thickness of the sun's atmosphere. It did not strike anybody at first to throw an image of the sun on the slit of a spectroscope and see if there were a difference; the inquiry was, however, made in another way.

In 1836, Professor Forbes began it by taking advantage of an annular eclipse of the sun at Edinburgh, during which, of course, only the light from the edge reached us, the centre being cut off by the dark moon.

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[1] *Loc. cit.* p. 529.

In describing the result of his observation in the *Philosophical Transactions*,[1] mindful of Brewster's work, he first shows that the deficient rays cannot be due to any absorptive action of the glass prisms employed, since whatever material is employed and whatever the length of the path of the light through the prisms the lines remain constant. He then refers to Brewster's discovery of the lines due to the absorption of the earth's atmosphere, but points out that such lines are neither numerous nor important, as compared with the great mass of solar lines, and that if the lines were all or chiefly due either to the absorptive action of the earth's atmosphere or of any matter which may exist in the planetary spaces, we should have the same lines exhibited in the spectra of the fixed stars.

Reference is also made to Brewster's idea, suggested by his observations of the absorption spectrum of nitrous acid, that the solar light is originally complete and that the deficient rays have been stopped—absorbed—in passing through the sun's own atmosphere, which might be supposed to contain nitrous acid or some similar gas as a constituent.

On this point the following note is appended to Forbes's paper:—

> "I do not know with whom the idea of the absorptive action of the sun's atmosphere originated. The editors of the *London and Edinburgh Philosophical Magazine* (December, 1836) have, however, referred me to the mention of it in Sir John Herschel's writings, particularly his *Elementary Treatise on Astronomy*, from which I extract the following remarkable passage: 'The prismatic analysis of the solar beam exhibits in the spectrum a series of fixed lines totally unlike those of any known terrestrial flame. This may hereafter lead us to a clearer insight into its origin. But before we can draw any conclusions from such an indication we must recollect that previous to reaching us it has undergone the whole absorptive action of our atmosphere, as well as of the sun's. Of the latter we know nothing and may conjecture everything. . . . It deserves

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[1] *Phil. Trans.* 1836, p. 450 *et seq.*

inquiry whether some or all of the fixed lines observed by Wollaston and Fraunhofer, may not have their origin in our own atmosphere. Experiments made on lofty mountains or the cars of balloons on the one hand, and on the other with reflected beams which have been made to traverse several miles of additional air near the surface, would decide this point. The absorptive effect of the sun's atmosphere, and possibly also of the medium surrounding it (whatever it be), which resists the motion of comets, cannot be thus eliminated.' Herschel's *Astronomy*, p. 212, *note*. See also his Essay on Light, *Encyclopædia Metropolitana*, art. 505. The object of the experiment now described is to show a method of elimination which applies, at least, to the sun's atmosphere."

To test this view, then, Prof. Forbes determined to take advantage of the opportunity afforded him by the eclipse to examine the spectrum of the edge of the sun without any interference from the central rays, for, he argued, if the absorption is due to the solar atmosphere, it ought to be more marked at the edge of the sun, from which the light has to pass through a thicker stratum of atmosphere than at the centre, from which the rays proceed vertically and through a minimum of atmosphere. That there is a general darkening of the sun's limb is a well-known fact that can be observed at any time by examining the sun through a dark glass or a fog. Prof. Forbes was probably aware of this, and expected to find the darkening accompanied by an increased selective absorption.

The result of the observation was that as the eclipse progressed, and the proportion of lateral to central light consequently increased, no change whatever was observed in the number, position, or thickness of the lines, and from this observation Prof. Forbes concluded that—

> "This result proves decisively that the sun's atmosphere has nothing to do with the production of this singular phenomenon."

His conclusion, then, was at variance with that held by his predecessors, and we now know that Prof. Forbes's conclusion was wrong. We have, however, not yet finished with the paper. At the close of it he refers to a matter which is of considerable interest, showing, as it does, how near one may be to a most important discovery and yet miss it. I give it in the Professor's own words:—

> "Had the weather proved unfavourable for viewing the eclipse, I intended to have tried the experiment by forming an image of the sun by using lens of long focus, stopping alternately by means of a screen the exterior and central moiety of his rays, and restoring the remainder to parallelism by means of a second lens, then suffering these to fall upon a slit as before. The result of my experiment during the eclipse seemed, however, so decisive as to no marked change being produced at the sun's edges that I have thought it unnecessary to repeat it."

It will be seen that Forbes all but introduced the method of localisation of solar phenomena, which is brought about by throwing an image of the sun on the slit-plate of the spectroscope, whereby we are enabled to examine the spectrum of any particular part of disc or limb at pleasure. Yet, although Forbes came so near applying this principle, which has turned out of so high importance, we had to wait until 1866 before it was actually carried out.

The year 1842 marked another very great advance in our knowledge of the solar spectrum. While experimenting with photography,[1] Becquerel obtained by its means a representation of the whole spectrum with nearly all the lines

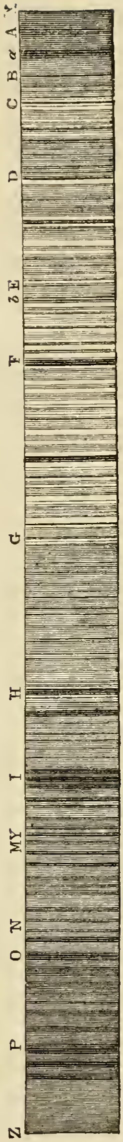

FIG. 23.—Reduced copy of Becquerel's photograph of the complete solar spectrum taken in 1842.

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[1] *Bibliothèque Universelle de Genève*, t. 39-40, p. 341 (1842).

mapped by Fraunhofer from the extreme red to the violet, and *besides these, certain new lines right beyond the visible spectrum, in the ultra violet.*

In 1843 Draper[1] accomplished a somewhat similar result in what he called a "tithonographic representation of the solar spectrum."[2] In this he showed certain lines in the extreme visible blue and extreme red, and beyond the visible red he found other lines which even Becquerel had not seen; but his photograph gave no record of any lines in the yellow, orange, or green.

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[1] *Phil. Mag.* vol. xxii. p. 360 (1843). For his earliest work, see *Journal Franklin Institute,* 1837.

[2] Here we had a foretaste of what photography has since done for us in solar studies.

CHAPTER V.

THE FRAUNHOFER LINES EXPLAINED.

WHILE the strange dark lines in the solar spectrum were attracting attention, various attempts were made to account for their existence, and many new facts throwing light on the matter were discovered.

We have seen, for instance, that Brewster and Herschel in their work came across substances which seemed to give, so to speak, an artificial solar spectrum, that is, a spectrum crossed by dark lines.

I have been favoured by Dr. Gladstone with the following extract from Brewster's note-book, dated St. Andrew's, Oct. 28th, 1841:—

> "I have this evening discovered the remarkable fact that in the combustion of nitre upon charcoal there are definite bright rays corresponding to the double lines of A and B, and the group of lines *a* in the space AB. The coincidence of two yellow rays with the two deficient ones at D, with the existence of definite bright rays in the nitre flame, not only at D, but also at A, *a*, and B, is so extraordinary that *it indicates some regular connection between the two classes of phenomena.*"

Brewster had already indicated how close the connection might be between absorption phenomena going on here and at the sun. Now he indicates a possible connection between radiation phenomena here and absorption at the sun.

The bright double yellow line observed by Fraunhofer and Brewster is the bright line of sodium vapour referred to in the previous chapter.

In 1849 this work was continued by Foucault,[1] who used a new method of work by getting a new light-source—the voltaic arc—produced by the passage of an electric current between charcoal poles charged with salts of different substances. With this new engine he also investigated the double yellow line.

> "The spectrum is marked, as is known, in its whole extent by a multitude of irregularly grouped luminous lines; but among these may be remarked a double line situated at the boundary of the yellow and orange. As this double line recalled, by its form and situation, the line D of the solar spectrum, *I wished to try if it corresponded to it*, and in default of instruments for measuring the angles I had recourse to a particular process.
>
> "I caused an image of the sun, formed by a converging lens, to fall on the arc itself, which allowed me to observe at the same time the electric and the solar spectrum superposed; I convinced myself in this way that the double bright line of the arc coincides exactly with the double dark line of the solar spectrum.
>
> "This process of investigation furnished me matter for some unexpected observation. It proved to me, in the first instance, the extreme transparency of the arc, which occasions only a faint shadow in the solar light. *It showed me that this arc, placed in the path of a beam of solar light*, absorbs *the rays* D, so that the above-mentioned line D of the solar light is considerably strengthened when the two spectra are exactly superposed.
>
> "When, on the contrary, they jut out one beyond the other, the line D appears darker than usual in the solar light, and stands out bright in the electric spectrum, which allows one easily to judge of their perfect coincidence. *Thus the arc presents us with a medium which emits the rays* D *on its own account, and which at the same time absorbs them when they come from another quarter.*"

Thus absorption is produced when we destroy the simplicity of the usual medium through which the light passes. Instead of ether only, it has to traverse ether plus certain kinds of matter contained in the arc. This association leads to certain vibrations of the ether being used up by the matter, and so the residue only reaches the eye.

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[1] *L'Institut*, Feb. 7, 1849. Translated by Professor Stokes in *Phil. Mag.* vol. xix. p. 194.

Foucault then continues :—

> "To make the experiment in a manner still more decisive, I projected on the arc the reflected image of one of the charcoal points, which, like all solid bodies in ignition, gives no lines; and under these circumstances the line D appeared to me as in the solar spectrum."

In 1849, then, we get the statement made that a vapour could absorb the same rays that it radiated, and that the light radiated by sodium vapour occupied the same position in the spectrum as some of the light which was absorbed at the sun. But why? Was there, then, sodium vapour in the sun? This conclusion could not be accepted until a valid explanation could be given. If the experiment had been made with *cool* sodium vapour—which might, as we now know, easily have been done—the thing might have been at once conceded, but the fact that the same substance could produce a bright line under one condition and a dark one under another was difficult to imagine.

An explanation, however, was not long forthcoming. One was first given (though not published) by Prof. Stokes, about the year 1852.

The observational and experimental foundation on which Stokes based his explanation has thus been stated by Sir William Thomson :—[1]

> "1. The discovery by Fraunhofer of a coincidence between his double dark line, D, of the solar spectrum and a double bright line which he observed in the spectra of ordinary artificial flames.
>
> "2. A very rigorous experimental test of this coincidence by Prof. W. H. Miller, which showed it to be accurate to an astonishing degree of minuteness.

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[1] Presidential Address, British Association Meeting, 1871.

"3. The fact that the yellow light given out when salt is thrown into burning spirits consists almost solely of the two nearly dentical qualities which constitute that double bright line.

"4. Observations made by Stokes himself which showed the bright line D to be absent from a candle flame when the wick was snuffed clean, so as not to project into the luminous envelope, and from an alcohol flame when the spirit was burned in a watch-glass; and,

"5. Foucault's admirable discovery, already referred to, that the voltaic arc between the charcoal points is a medium which emits the rays D on its own account, and at the same time absorbs them when they come from another quarter."

The conclusions at which Stokes arrived were:—

"1. That the double line D, whether bright or dark, is due to the vapour of sodium.

"2. That the ultimate atom of sodium is susceptible of regular elastic vibrations, like those of a tuning-fork or of stringed musical instruments; that, like an instrument with two strings tuned to approximate unison, or an approximately circular elastic disc, it has two fundamental notes or vibrations of approximately equal pitch; and that the periods of these vibrations are precisely the periods of the two slightly different yellow lights constituting the double bright line D.

"3. That when vapour of sodium is at a high enough temperature to become itself a source of light, each atom executes these two fundamental vibrations simultaneously, and that therefore the light proceeding from it is of the two qualities constituting the double bright line D.

"4. That when vapour of sodium is present in space across which light from another source is propagated, its atoms, according to a well-known principle of dynamics, are set to vibrate in either or both of these fundamental modes, if some of the incident light is of one or other of their periods, or some of one and some of the other: so that the energy of the waves of these particular qualities of light is converted into thermal vibrations of the medium and dispersed in all directions, while light of all other qualities, even though very nearly agreeing with them, is transmitted with comparatively no loss.

"5. That Fraunhofer's double dark line, D, of solar and stellar spectra, is due to the presence of vapour of sodium in atmospheres surrounding the sun and those stars in whose spectra it has been observed.

"6. That other vapours than sodium are to be found in the atmosphere of sun and stars by searching for substances producing in the spectra of artificial flames bright lines coinciding with other dark lines of the solar and stellar spectra than the Fraunhofer line D."

Although Professor Stokes unfortunately did not publish his theory—I say unfortunately because valuable time has been lost—the world was not long in ignorance of a matter of such general interest, for in 1853 the idea was published by the celebrated Ångström.[1]

In his memoir, illustrating the absorption of light, he made use of a principle already propounded by Euler (in his *Theoria lucis et caloris*), that the particles of a body, in consequence of resonance, absorb principally those ethereal undulatory motions which have previously been impressed upon them. He also endeavoured to show that a body in a state of glowing heat emits just the same kinds of light and heat which it absorbs under the same circumstances. He further, following in this respect the steps of Foucault, undertook a series of researches on the electric arc—just such researches as those Stokes had only suggested—and found that in many cases the Fraunhofer lines were an inversion of bright lines which he observed in the spectra of various metals.[2] Of these we shall have much to say in the sequel.

Early in 1858 Balfour Stewart independently discovered the law which binds together radiation and absorption, establishing it experimentally and also theoretically as an extension of

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[1] "Öptiska Undersokningar," *Trans. Royal Academy of Stockholm*, 1853. Translated in *Phil. Mag.*, Fourth Series, vol. ix. p. 327.

[2] See *Phil. Mag.*, Fourth Series, vol. xxiv. pp. 2, 3: *Monatsbericht*, 1859, p. 662.

Prevost's law of exchanges, in the case of the heat-rays, and generalising his conclusion for all rays.[1]

In October of the year 1859, Kirchhoff established experimentally the same law for light-rays. His first announcement, dated Heidelberg, 20th October, 1859, read before the Berlin Academy on the 27th,[2] must here be given *in extenso*:—

> "On the occasion of an examination of the spectra of coloured flames not yet published, conducted by Bunsen and myself in common, by which it has become possible for us to recognise the qualitative composition of complicated mixtures from the appearance of the spectrum of their blowpipe flame, I made some observations which disclose an unexpected explanation of the origin of Fraunhofer's lines, and authorise conclusions therefrom respecting the material constitution of the atmosphere of the sun, and perhaps also of the brighter fixed stars.
>
> "Fraunhofer had remarked that in the spectrum of the flame of a candle there appear two bright lines which coincide with the two dark lines D of the solar spectrum. The same bright lines are obtained of greater intensity from a flame into which some common salt is put. I formed a solar spectrum by projection, and allowed the solar rays concerned, before they fell on the slit, to pass through a powerful salt flame. If the sunlight were sufficiently reduced, there appeared in place of the two dark lines D, two bright lines; if, on the other hand, its intensity surpassed a certain limit, the two dark lines D showed themselves in much greater distinctness than without the employment of the salt flame.
>
> "The spectrum of the Drummond light contains, as a general rule, the two bright lines of sodium if the luminous spot of the cylinder of lime has not long been exposed to the white heat; if the cylinder remains unmoved these lines become weaker, and finally vanish altogether. If they have vanished, or only faintly appear, an alcohol flame into which salt has been put, and which is placed between the cylinder of lime and the slit, causes two dark lines of remarkable sharpness and fineness, which in that respect agree with the lines D of the solar spectrum, to show themselves in their stead.

---

[1] *Edinburgh Transactions*, 1858–9.

[2] See translation, by Professor Stokes, in the *Phil. Mag*, Fourth Series, vol. xix. p. 195.

Thus the lines D of the solar spectrum are artificially evoked in a spectrum in which naturally they are not present. . . .

"I conclude from these observations, that coloured flames in the spectra of which bright sharp lines present themselves so weaken rays of the colour of these lines, when such rays pass through the flames, that in place of the bright lines dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting. *I conclude further, that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth exist in consequence of the presence, in the incandescent atmosphere of the sun, of those substances which in the spectrum of a flame produce bright lines at the same place.* We may assume that the bright lines agreeing with D in the spectrum of a flame always arise from sodium contained in it; the dark line D in the solar spectrum allows us, therefore, to conclude that there exists sodium in the sun's atmosphere. . . .

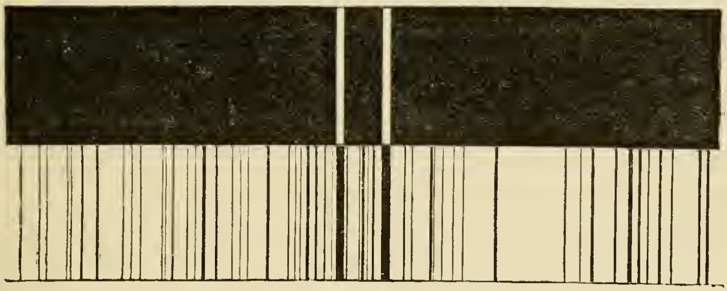

FIG. 24.—Diagram showing the solar spectrum and the bright double line D of sodium.

"In the course of the experiments which have at present been instituted by us . . . . a fact has already shown itself which seems to us to be of great importance. The Drummond light requires, in order that the lines D should come out in it dark, *a salt flame of lower temperature.* The flame of alcohol containing water is fitted for this, but the flame of Bunsen's gas lamp is not. With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves. . . . ."

The point marked with italics is the most important part of

Kirchhoff's work. He insisted that the absorber must be cooler than the radiator.

Immediately after the publication of this note of Kirchhoff's, Stewart[1] explained, in extension of his former work on the theory of exchanges, why it was that a salt flame of lower temperature was required to darken the D lines, pointing out that it was a phenomenon analogous to that presented when a piece of ruby glass is heated in the fire. So long as the ruby glass is cooler than the coals behind it the light given out is red, because the ruby glass stops the green; the green light is therefore analogous to the line D which is given out by an alcohol flame into which salt has been put. Should, however, this ruby glass be of a much higher temperature than the coals behind it, the greenish light which it radiates overpowers the red which it transmits, so that the light which reaches the eye is more green than red. This is precisely analogous to what is observed when a Bunsen's gas flame with a little salt is placed in front of the Drummond light, when the line D is no longer dark but bright.

In a paper dated Heidelberg, January, 1860, and published in *Poggendorff's Annalen*,[2] Kirchhoff proves from theoretical considerations that the spectrum of an incandescent gas must become reversed (that is, the bright lines become changed into dark ones) when a source of light of sufficient intensity, giving a continuous spectrum, is placed behind the luminous gas.

He also describes an extended series of observations similar to those recorded in his first paper, and in a note states that in conjunction with Professor Bunsen he had succeeded in reversing the brighter lines of potassium, calcium, strontium, and barium, by exploding, before the slit, mixtures of their chlorates with milk sugar during the passage of the sun's rays.

In discussing the bearing of his observations made up to that time on the constitution of the sun he says:—

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[1] "On the Theory of Exchanges and its Recent Extension."—*B.A. Reports*, 1861.

[2] Translated by Mr. F. Guthrie, in *Phil. Mag.* vol. xx. July, 1860, p. 1.

"Imagine a body of very high temperature, in whose spectrum the double line D does not appear, *surrounded by a gaseous atmosphere of somewhat lower temperature.* If sodium be present in the latter, the spectrum of the whole system so constituted will contain the double line D. *From the occurrence of these lines the presence of sodium in the atmosphere may therefore be concluded.* Now, the sun is undoubtedly a body of this description; and therefore, from the occurrence of the lines D in the solar spectrum, the presence of sodium in the sun's atmosphere may be concluded."

. . . .

"What has been stated concerning sodium is equally true of every other substance which, when placed in a flame of any sort, produces bright lines in its spectrum. If these lines coincide with the dark lines of the solar spectrum, the presence in the sun's atmosphere of the substances which produce them must be concluded, provided always that the lines in question cannot have their origin in the atmosphere of the earth. In this way means are afforded of determining the chemical constitution of the sun's atmosphere; and the same method even promises some information concerning the constitution of the brighter fixed stars."

In a subsequent paper[1] describing the result of the combined work of Kirchhoff and Bunsen, it is stated that, to obtain further confirmation of the observations recorded in Kirchhoff's first paper, the authors produced a bright continuous spectrum by the intense ignition of a platinum wire, and interposed between it and the slit a flame of weak alcohol containing common salt. The dark D line was then seen most distinctly.

They also found that the dark D line could be produced by the interposition, between the incandescent wire and the slit, of sodium amalgam heated to boiling, and they pointed out the special importance of this in showing that sodium vapour at a temperature much below that at which it becomes luminous, exerts its absorptive power at exactly the same point of the spectrum as it does at the highest temperatures which we can produce, or at the temperatures existing in the solar atmosphere.

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[1] *Phil. Mag.*, Fourth Series, vol. xx. p. 108, August, 1860.

It has been necessary in dealing with this, so to speak, crucial epoch in the history of our subject to give as far as possible the *ipsissima verba* of the workers whose genius we have to thank for a series of researches which has not only enabled us to study the chemistry of the sun, but has given us as its first tremendous outcome the conclusion that the sun and earth have the same chemical constituents. Where some, at all events, might have anticipated a new world of matter, we find likeness to the old.

With regard to this conclusion it is not too early to make the following remarks on the views concerning the chemical elements held when the spectroscope was introduced.

That instrument placed us in presence of phenomena produced by higher temperatures than had ever been employed before. The elements either did or did not bear that temperature without dissociation, and the spectra observed were, therefore, either the spectra of the elements themselves, or of their constituents.

Similarly the sun must either give us the spectra of the elements contained in it or the spectra of their constituents.

The similarity, therefore, of solar and terrestrial spectra, even if established, cannot be regarded as a final argument in favour of the elementary nature of the bodies which Lavoisier had considered elemental. This point, in fact, was never, so far as I know, raised.

On this subject I am permitted to print a memorandum which has been given to me by my friend, Dr. Hodgkinson:—

> The great point in Lavoisier's teaching, affecting the chemistry of his and our own time, was the recognition of the compound nature of some bodies, and the so-called simple nature of others; and that these compounds can be reduced to the simple state, or elemental condition, of matter.
>
> The history of chemistry is one of a development of ideas and theories, more so than in any other science; a given theory or idea

opens up a road much too wide to be completely filled by it in its original form, and a more comprehensive one must take its place.

The great chemist was careful whilst tabulating the irresolvable substances in his time into elements, to state that it is probable we do not know the elements themselves, but rather should understand that by analysis we have arrived at a limit—at terminal constituents—and that for our purpose all such bodies may be looked upon as simple.

But this idea of "elements" as the basis of matter took very firm root, and while it has contributed immensely to the growth of chemistry it cannot, latterly at least, be considered as all-sufficient.

At the time, however, of Kirchhoff's earlier work the notion of the elements being absolutely elemental appears to have been held very firmly, and no doubt seems to have been felt that the numerous lines forming the spectrum of any one so-called element were other than peculiar to that element alone. In other words, no hesitation was felt at ascribing a most complex spectral appearance to a simple element, regarded as such from its chemical irresolvability under ordinary circumstances.

The idea that the "elements" were final substances or forms of matter was first shaken by the discovery and isolation of groups of substances whose chemical behaviour was exactly analogous to that of other substances regarded as elemental. The full meaning and importance of this idea of compound radicals or groups has perhaps only been fully recognised during the past decade, it certainly was not at the time of Kirchhoff's first investigations on spectra. On the contrary, the "finality of elementary matter" notion was much strengthened by the coincidence of solar with terrestrial spectral results.

With our present knowledge of the behaviour, chemical and physical, of the groups we call radicals, especially those classed as organic; and our extended notions of allotropism, and isomeric and polymeric forms of bodies; it is very unlikely, were spectrum analysis to be discovered now, that the notion of an elemental form of matter would be ascribed to substances giving complex spectra, but rather having regard to the analogies above referred to, the phenomena would be ascribed to groups of elements the peculiarity of which is that they act, within certain ranges of condition, as individuals—as complexes of atoms of *wider* range of existence—to the limits of which we have not as yet attained.

In the case of many compounds of carbon alone, we are able to trace them through their entire range of existence, with others only partially so. In all cases the complex groups formed from simpler ones have related but not identical properties. In other substances this range of condition of existence passes gradually out of our reach, but the phenomena remain the same in kind. Atomic combination or condensation is probably a cause of what is termed the "allotropic state" of some forms of matter. It is strikingly in analogy with polymerization, but its investigation is at present out of our range.

Let us now dwell for a little on the principle by the application of which the great stride we have chronicled has been rendered possible.

While in the giving out of light we are dealing with molecular vibration taking place so energetically as to give rise to luminous radiation, absorption phenomena afford us evidence of this motion of the molecules when their vibrations are far less violent.

The molecules are so apt to vibrate each in its own period, that they will even take up vibrations from light which is passing among them, provided always that the light thus passing among them contains the proper vibrations.

Let us try to get a mental image of what goes on. There is an experiment in the world of sound which will help us.

If we go into a quiet room where there is a piano, and sing a note and stop suddenly, we find that note echoed back from the piano. If we sing another note we find that it also is re-echoed from the piano. How is this? When we have sung a particular note we have thrown the air into a particular state of vibration. One wire in the piano was competent to vibrate in harmony with it. It did so, and vibrating after we had finished, kept on the note.[1]

---

[1] The following extract will show that this has been long known :—

"How doth musicke amaze us, when of sound discords she maketh the sweetest Harmony? And who can show us the reason why two Basons, Bowles, Brasse

This principle may be illustrated in another and very striking manner by means of two large tuning-forks mounted on sounding-boxes and tuned to exact unison. One of the forks is set in active vibration by means of a fiddle-bow, and then brought near to the other one, the open mouths of the two sounding-boxes being presented to each other to make the effect as great as possible.

After a few moments, if the fork originally sounded is damped to stop its sound, it will be found that the other fork has taken up the vibration and is sounding, not so loudly as the original fork was, but still distinctly. If the two forks are not in unison, no amount of bowing of the one will have the slightest effect in producing sound from the other.

Again, suppose we have a long room, and a fiddle at one end of it, and that between it and an observer at the other end of the room there is a screen of fiddles, all tuned like the solitary one, we can imagine that in that case the observer would scarcely hear the note produced upon any one of the open strings of the solitary fiddle. Why? The reason is that the air-pulses set up by the open strings of this fiddle, in unison with all the other fiddles, would set all the other open strings in vibration, and upon the principle that you cannot eat your cake and save it too, the air-pulses set in motion by the vibration of the fiddle cannot set all those strings vibrating and still pass on to one's ear at the other end of the room as if nothing had happened.

The work, in fact, which the air, the medium in this case,

---

pots, or the like of the same bignesse; the one being full, the other empty, shall, stricken, be a just *Diapason* in sound one to the other: or that there should be such sympathy in sounds, that two Lutes of equal size being laid upon a Table, and tuned unison, or alike in the Gamma, G, sol, re, ut, or any other string; the one stricken, the other untouched shall answer it?"—*The Compleat Gentleman Fashioning him absolut in the most necessary and commendable Qualities concerning Minde or Body, that may be required in a Noble Gentleman. By Henry Peacham, M.A., Sometime of Trinitie Colledge in Cambridge.* Third edition, 1661.

would have to do to make the sound-waves passing along it audible to the observer, would be locally done, so to speak, upon the screen of fiddles; the work done would decrease the amplitude of the sound-waves, and the sound would be weakened.

Now apply this to light. Suppose we have at one end of a room a vivid light-source giving us all possible waves of light from red to violet. This we may represent as before by

V I B G Y O R

Also suppose that we have in the middle of the room a screen of molecules capable of emitting yellow light,

Y

What will happen? Will the light come to our eyes exactly as if the molecules were not there? No; it will not. There will be a difference. What, then, will be the difference?

Experimentally we find that the molecules which give out yellow light, have kept for their own purpose—filched, so to speak, from the light passing through them—the particular vibrations which they want to carry on their own motions, and we shall have

V I B G   O R

as a result; the light comes to us minus the vibrations which have thus been utilised, as we may put it, by the screen of vapour.

We have, in fact, an apparently dark space which may be represented in this way:

V I B G **Y** O R

In the spectroscope we see what would otherwise be a continuous spectrum, with a dark band across the yellow absolutely identical in position with the bright band observed when the molecules of the vapour of which the screen is composed radiated light in the first instance. It is not, however, a case of absolute blackness or absence of that particular ray, for the molecules are set in vibration by the rays they absorb, and therefore give out some light, but it is so feeble as to appear black by contrast with the very much brighter rays coming direct from the original source.

The law which connects radiation with absorption and at once enables us to read the riddle set by the sun and stars, is then

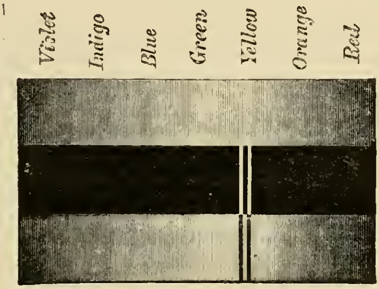

Fig. 25.—Diagram illustrating the graphical formulæ employed on the opposite page. Above is the continuous spectrum given by a source of white light. Next below is the radiation spectrum (consisting of two bright lines) of sodium; and at the bottom is seen the effect of the interposition of the screen of sodium vapour between the white light and the eye. The lines are still seen occupying exactly the same positions as before, but they are reversed, *i.e.* they appear black on a background of continuous spectrum.

simply the law of sympathetic vibration. Kirchhoff's explanation should now present no difficulty. The light orginally given out from the sun is white, and would give a continuous spectrum, but before it reaches us it has to pass through a screen consisting of the comparatively cool solar atmosphere; and if we imagine that atmosphere to contain sodium, iron, &c., among the vapours, those vapours will behave just as our hypothetical screen of molecules behaved; they will abstract from the white light passing through them those rays the rates of vibration of which synchronize with their own; the light will reach us deficient in those rays and will present a dark line for each ray so absorbed.

At the same time that the Fraunhofer lines in the solar spectrum were shown to be in the main due to the presence of vapours in the sun, the chemical nature of which vapours we could recognise by this means; the idea of Brewster that some were produced by an absorption due to the atmosphere of the earth, was fully confirmed. This fact, as we have seen, was first suspected because certain lines were found to vary with the height of the sun. They were very faint when the sun was high, and got thicker and darker as the sun approached the horizon.

In continuation of Brewster's work, Professor Piazzi Smyth[1] experimented at the Peak of Teneriffe in 1856. At one station, Guajara, 8,903 feet above the sea, he found that the spectrum when the sun was near the zenith began between *a* and B, and with the exception of C and D, contained no marked lines between B and E. When the sun was near the horizon the spectrum commenced outside A, and contained many powerful groups of lines between A and B; the thickness of B was quadrupled, while C remained unaffected; a number of fine lines between C and D were greatly thickened, and many new lines appeared beyond D. So striking was the change that at sunrise he could see the spectrum lengthen out and the new lines grow visibly under his eye.

At the sea-level the zenith spectrum approximated to the horizon spectrum as observed on the mountain, but there were some minor points of difference.

At the violet end, Professor Smyth found that with a high sun the spectrum extended beyond H, but when the sun was low it terminated between H and G. At the sea-level it stopped immediately beyond H, the two bars of which were nebulous; while at Alta Vista, another station, 10,702 feet high, the H lines had lost their nebulosity and the spectrum was visible beyond H to three times the distance of its bars asunder.

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[1] *Phil. Trans.* 1858, part ii. pp. 503–507.

Singularly enough, Brewster [1] himself, in 1859, when the truth of his view of the solar origin of those Fraunhofer lines which had not been traced to atmospheric absorption was being confirmed on all sides, appeared less satisfied with it, and Dr. Gladstone, who was now associated with him, endeavoured to show that they also might have a telluric origin. They suggested the possibility of the lines being due to interference,[2] as Brewster had observed analogous lines and bands in portions of decomposed glass consisting of numerous films. But not being quite satisfied with this they attempted to ascertain whether the earth's atmosphere had any share in their production. Dr. Gladstone therefore, in 1859,[3] examined the light of the lighthouse at Beachy Head from Shoreham and Worthing (twenty-five and twenty-seven miles respectively), expecting to find the Fraunhofer lines. The only result of this observation, however, was that the spectrum terminated at C and F, and no dark lines appeared, and they concluded, therefore, that the origin of those fixed lines must still be considered an undecided question.

Hoping to obtain some confirmation of the telluric origin of the variable lines of the solar spectrum, Dr. Gladstone, in 1861, compared them with the bright lines emitted by the different constituents of the atmosphere when sufficiently heated.[4] As data for this comparison, he took Ångström's maps of the spectra of oxygen, nitrogen, hydrogen, carbonic acid, &c., obtained, by means of an electric spark, and Plucker's maps of the spectra of the same and other gases, in Geissler tubes. To obtain the spectrum of aqueous vapour he employed the oxyhydrogen flame, and of carbonic acid, a flame of carbonic oxide.

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[1] *Phil. Trans.* 1860, part i. pp. 157-159.

[2] *Ibid.* p. 159, note.

[3] *Ibid.* p. 159.

[4] *Proc. Roy. Soc.* June 20th, 1861.

The result of this comparison was, that there was no accordance between the bright lines of the gases and the atmospheric lines, H alone being inconclusive.

From the variability of the appearance of the atmospheric lines from time to time when the sun is on the horizon, and from the fact that one of the lines had been observed during a shower, and the most prominent lines during a fog, they had been referred to aqueous vapour, but Dr. Gladstone considered this could scarcely be, for they are not exhibited by the sun's rays passing through the edge of a cloud unless near the horizon, and they appeared near sunset when, in frosty weather, the aqueous vapour is reduced to a minimum, though they are not seen when the sun is higher up in the heavens on a warm day.

In 1864, Janssen, imitating Gladstone's method of work, observed the spectrum of a large bonfire through a thickness of atmosphere of 21,000 mètres over the Lake of Geneva, and in this experiment was more fortunate than Dr. Gladstone, for he saw many lines, though close to the fire there was no absorption whatever.

By a subsequent experiment he proved that the lines were really due to *aqueous vapour*. In this he used a long length of gas main which the Paris Gas Company had placed at his disposal, and filled it with steam, taking precautions to keep the temperature high and the glass ends transparent. At one end of this he placed a bright flame, at the other he observed the light by means of a spectroscope after it had traversed the whole length of tube. He thus obtained a spectrum which was the exact equivalent of that which is superadded on to the true solar one, and which becomes most marked when, the sun being low, there is the greatest possible thickness of our atmosphere and its contained aqueous vapour, to give rise to a larger amount of absorption.

In 1867, Ångström,[1] referring to this note of Janssen's, stated

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[1] *Phil. Mag.*, 1867, p. 76.

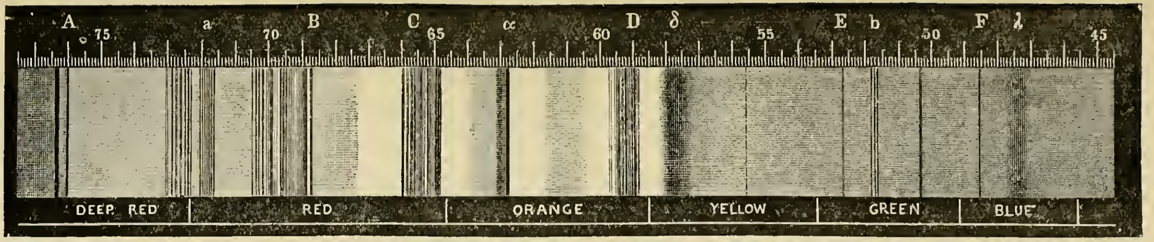

FIG. 26.—The Telluric Lines. Taken from the Atlas to Ångström's *Spectre Normal*. The observations shown extend over the whole of the visible spectrum. The red part of the spectrum is to the left.

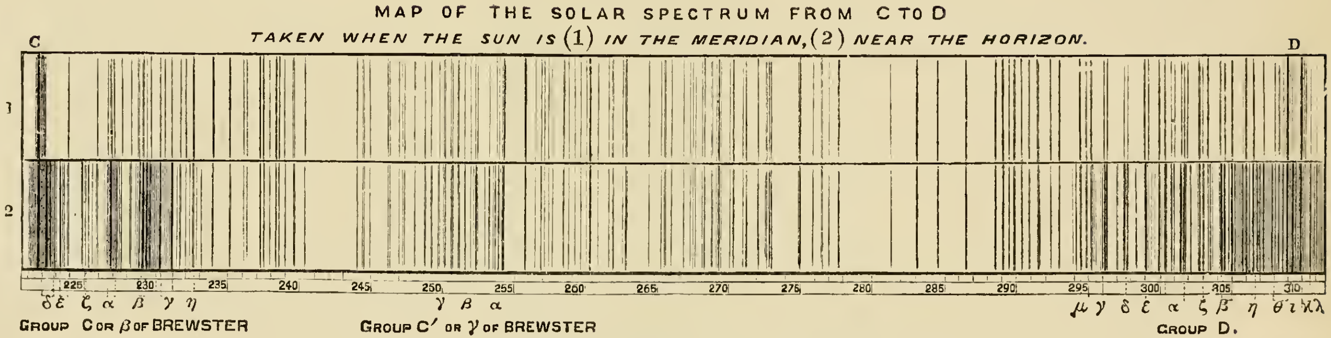

FIG. 27.—Copy of Janssen's Map of the Telluric Lines from C to D.

that he could not agree with that investigator in assigning to aqueous vapour the whole of five groups of lines extending from D to A, and including the groups A and B. These groups, Ångström expressed his belief were truly telluric lines, but he denied that they are due to aqueous vapour, relying upon the fact that during very cold weather (once as low as —27° C.) in January, 1864, the telluric lines near D, C, and *a*, as well as those from *a* to B, had almost entirely disappeared: while the groups A and B, and a third about midway between B and C, were very intense. He therefore assigned these three groups to some compound gas, carbonic acid for instance, from their resemblance to the spectra of compound gases, and more especially of metallic oxides.

Fig. 27 is a copy of Janssen's map of the telluric lines between D and C.

These lines have since been made the subject of many beautiful researches by Smyth and others, but now that we have seen that they belong truly to our atmosphere they possess no longer the same interest for us in this inquiry, and we must dismiss them from our consideration, because they deal with the earth's chemistry and tell us nothing touching the chemistry of the sun.

CHAPTER VI.

KIRCHHOFF'S MAP AND WORK.

In the course of the last chapter, we found ourselves, as a result of all the work done up to 1860, face to face with the statement that the atmosphere of the sun contained, if it was not entirely built up of, substances known to terrestrial chemistry; and further, that our knowledge, of which this was only the first foretaste, might be almost indefinitely extended by a minute study of the Fraunhofer lines in connection with the spectra of terrestrial elements.

To this work Kirchhoff now set himself with tremendous vigour, and in the years 1861 and 1862 communicated memoirs of priceless value to the Berlin Academy.[1]

All the time the early work described in the previous chapters was being carried on, the apparatus employed was undergoing a sort of evolution, while various new methods were being developed. The various parts of the spectroscope were being perfected and made more compact; glass got better; better surfaces were given to prisms; until in Kirchhoff's time it attained the dignity of a complete and distinct instrument. We may now then lay aside the provisional arrangement described in Chapter IV. to make ourselves thoroughly acquainted with the construction of the actual instrument, and its recent additions and modifications, especially those for measuring purposes, before we can follow the advances made by its aid.

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[1] *Researches on the Solar Spectrum and the Spectra of the Chemical Elements*, by G. Kirchhoff, Professor of Physics in the University of Heidelberg. A translation by Roscoe was published by Macmillan.

One common form will be gathered from Fig. 28, which represents a small instrument with one prism. On the left we have the collimator *b*, at the outer extremity of which is the fine slit, the width of which can be regulated to a nicety by a micrometer screw. The spectrum is observed by the telescope *f*, which is simply a small astronomical telescope of low magnifying power. *e* and *é* are Bunsen burners, in which the substances under examination can be volatilised while their spectra are superposed by means of the comparison prism to be described presently.

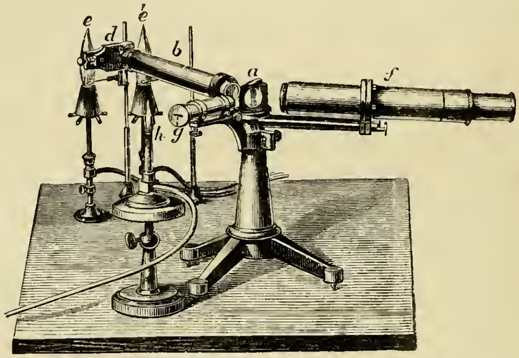

Fig. 28.—Spectroscope with reflected scale. *a*, prism; *b*, collimator; *d*, slit plate; *e*, *é*, Bunsen burners for volatilising salts; *f*, observing telescope; *g*, photographic scale, illuminated by gas flame, *h*.

Next a word as to the measurements necessary to enable us to map or compare spectra with each other. There are several ways in which this is commonly effected. In the instrument represented in Fig 28 there is a short tube carrying at its outer extremity a small photographic scale *g*, which is illuminated by a luminous gas flame *h*; the light passing from the scale is rendered parallel and thrown on the last surface of the prism by means of a lens in the tube carrying the scale, and is reflected by the last surface of the prism up the observing telescope, so that it is seen as a bright scale on the background formed by the spectrum under observation.

In another method of measurement, the telescope is attached to a movable arm, which can be directed to any part of the spectrum, and the outer edge of the circular stand along which the telescope moves is graduated with more or less minuteness, according to the precision in the position of the lines required in the research. In this method the line to be measured is brought into the centre of the field of view by the aid of cross wires in the eyepiece of the observing telescope, and the position of a pointer carried by the telescope is read off on the graduated arc.

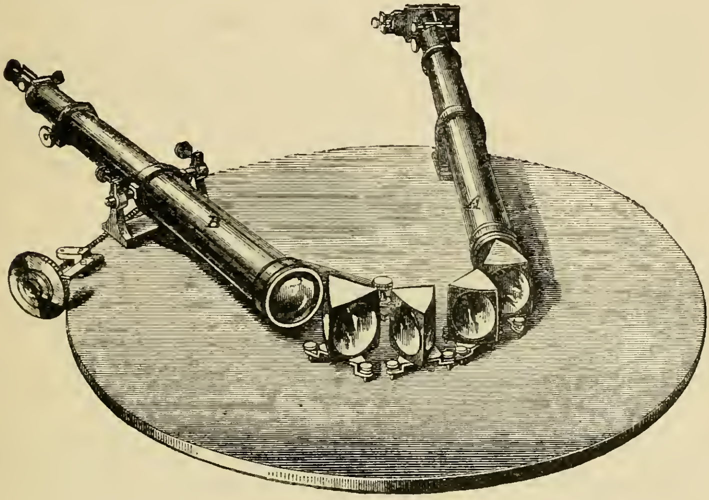

Fig. 29.—Steinheil's form of four-prism spectroscope. *A*, collimator; *B*, observing telescope.

There are many variations of these methods of measurement, but a description of them belongs rather to a special book on the spectroscope than to the present volume.

When delicate and detailed observations have to be made, the amount of dispersion afforded by one prism is insufficient, and has to be increased by the addition of more prisms. The way in which this is effected is shown in Fig. 29, which represents the instrument with which Kirchhoff made his elaborate maps.

By placing a mirror, or better still a heliostat, in front of this spectroscope, light from the sun, or a cloud illuminated by the sun in case the quantity of light which enters the instrument when turned directly towards the sun is too great to allow of easy observation, may be reflected into the instrument.

Using his measuring apparatus, Kirchhoff was enabled to construct his map showing the positions of the lines observed in the solar spectrum by moving his observing telescope along the spectrum, as it were, the telescope being furnished with a delicate cross wire, or some properly-contrived means for defining the exact position of each line. Indeed he prepared this map with the object of providing himself with a scale of extreme value for the future work which he then laid out for himself.

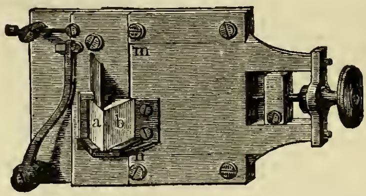

FIG. 30.—Steinheil's slit, showing reflecting prism (*a*, *b*).

The future work was this:—He wished to determine the positions of the bright lines given by the different chemical elements; having got this information, he wished to put the same question to the solar spectrum with regard to each of those elements as already had been done in the case of sodium. How then did he propose to do this? He made an addition to the slit of the spectroscope, such as was then employed. He put a prism in front of it, by means of which he could reflect into one-half of it the light from the incandescent vapour of the substance under examination, while the other half received the direct light of the sun. It was quite easy by this method to see in his observing telescope no longer the spectrum of the sun alone, but the spectrum of the sun together with the spectrum produced by the incandescent vapour of each of the chemical substances which he chose to experiment upon.

While the spectroscope was growing in perfection, great improvements were also made in the methods of producing incandescent vapours. Especially was this advance important in gradually pressing into service the use of higher and higher temperatures. When the earliest spectroscopic observations were made, the highest available temperature was practically as we have seen that afforded by a spirit-lamp. It is true that the very earliest workers, Wollaston and Fraunhofer, both employed electricity in their investigations, but they did not employ it in what we should to-day consider an available form. The invention of the Bunsen burner was the first step in advance. The first extensive employment of electricity in spectroscopic work was by Sir Charles Wheatstone (1835), who examined the spectra of mercury and various other metals obtained in a molten state by passing a rapid succession of electromagnetic sparks

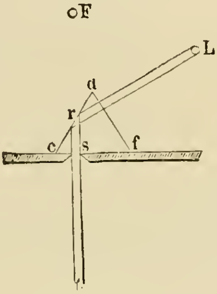

FIG. 31.—Path of light through comparison prism. *d*, *f*, *o*, prism ; L, light-source ; *r*, point of reflection ; *s*, slit ; F, light-source in front of slit.

obtained by an automatic make-and-break from the surface of the metal under examination.

A small oval disc was made to revolve, and in its revolution to act on one arm of a lever, beneath which a spring was placed like that to a flute key; this lever was fixed on a plate of sheet iron, having at its extremity a small cup to hold the mercury or other molten metal, and the other end of the lever was bent at right angles, so that as the oval disc revolved the bent end of the lever was alternately dipped into the metal and removed, and the spark always occurred precisely at the same point.[1]

In this way Wheatstone examined and mapped the spectra of mercury, zinc, cadmium, bismuth, tin, and lead.

This method was afterwards superseded by the introduction of the induction coil, an arrangement which produces high tension sparks of exceedingly high temperature, and has now come into general use for obtaining the spectra of metals. If necessary the temperature of the spark can be still further increased by introducing a Leyden jar into the circuit as shown in Fig. 32. This was the method of research employed by Kirchhoff.

As a specimen of Kirchhoff's work, we may consider his researches on the spectrum of iron. The point to determine was which of the bright lines corresponded with the dark Fraunhofer lines. Over the whole visible reach of the spectrum Kirchhoff carried this comparison. I will give one or two extracts from his paper. He says,[2] "It is especially remarkable that, coincident with the positions of all the bright iron lines which I have observed" (that is the bright lines from the vapour of iron, using two iron poles with an induction coil), "well-defined dark lines occur in the solar spectrum. By the help of the very delicate method of observation which I have employed, I believe that each coincidence observed by me between an iron line and a line in the solar spectrum, may be considered to be at least as well established as the coincidence of the sodium lines."

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[1] Paper read at the fifth meeting of the British Association, 1835, and printed in the *Chemical News*, vol. iii. p. 198, 1861.

[2] *Researches on the Solar Spectrum.* Roscoe's translation, part i. p. 18.

Then he shows, limiting his attention to sixty of the most defined iron lines in the region included in his map, that the probability that there is iron in the sun is about three trillions to one, dealing alone with the absolute matching of the positions of the lines recorded in the solar spectrum. Then he goes on to show that this probability of three trillions to one was rendered still greater by the fact that the brighter a given iron line is seen to be, the darker "as a rule"—and I beg attention to those words "*as a rule*"—"does the corresponding solar line appear. Hence this coincidence must be produced by some cause, and a cause can be assigned which affords a very perfect explanation of the phenomenon." He then gives the cause which has already been stated in Chapter V.

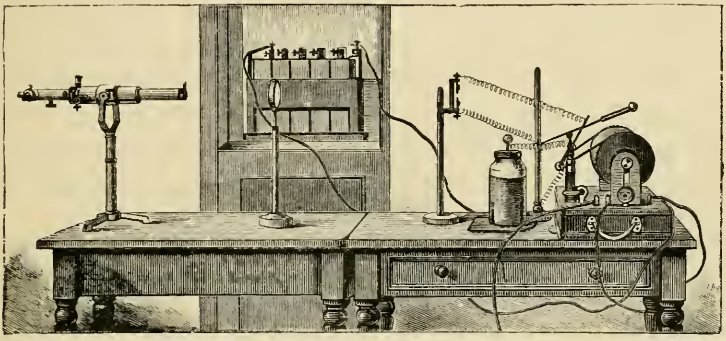

FIG. 32.—Arrangement of apparatus for observing the spectra of substances volatilised by the electric spark.

Now before I go further I must point out that there is a considerable assumption here. It is quite easy in an electric lamp to produce the vapour of a meteorite or of any of our terrestrial rocks, to throw their spectra on the screen, and to map them with considerable minuteness; and we say we have the spectrum of such and such a meteorite, or of such and such a rock. Similarly we can get the spectrum of iron in the same way, and we are considerably astonished at the wonderful similarity of the results thus obtained.

Now chemistry has advanced to a certain stage, and low temperature chemistry comes in and shows us that this meteorite or rock may be an excessively complicated substance. The same chemistry applied to iron shows that nothing can be done with it. But to say that iron cannot be broken up because low temperature chemistry fails to break it up is an *assumption,* for as we undoubtedly get the lines of the constituents of the rock, or of the meteorite, recorded in the spectrum, we may also be registering the lines of the constituents of iron; and it is fair to say this, because we know that in the electric spark we have a stage of heat at which at present no chemical experiment whatever has been made.

Passing on from that point however I will next consider somewhat more in detail that part of Kirchhoff's work which deals with the connection between the solar spectrum and the spectra of the chemical elements,[1] and again I quote from the translation of his memoir by Professor Roscoe:—

"As soon as the presence of one terrestrial element in the solar atmosphere was thus determined, and thereby the existence of a large number of Fraunhofer's lines explained, it seemed reasonable to suppose that other terrestrial bodies occur there, and that, by exerting their absorptive power, they may cause the production of other Fraunhofer's lines. For it is very probable that elementary bodies which occur in large quantities on the earth, and are likewise distinguished by special bright lines in their spectra, will, like iron, be visible in the solar atmosphere. This is found to be the case with calcium, magnesium, and sodium. The number of the bright lines in the spectrum of each of these metals is, indeed, small; but those lines, as well as the dark ones in the solar spectrum with which they coincide, are so uncommonly distinct that the coincidence can be observed with very great accuracy. In addition to this, the circumstance that these lines occur in groups, renders the observation of the coincidence of these spectra more exact than is the case with those composed of single lines. The lines produced by chromium also form a very characteristic group, which likewise coincides with a remarkable group of Fraunhofer's lines; hence I believe that I am justified in affirming the presence of chromium in the solar atmosphere. It appeared of great interest to determine whether the solar atmosphere contains nickel and cobalt, elements which invariably accompany iron in meteoric masses. The spectra of these metals, like that of iron, are distinguished by the large number of their lines; but the lines of nickel, and still more those of cobalt, are much less bright than the iron lines, and I was therefore unable to observe their position with the same degree of accuracy with which I determined the position of the iron lines. All the *brighter lines* of nickel appear to coincide with dark solar lines; the same was observed with respect to some of the cobalt lines, *but was not seen to be the case with other equally bright lines of this metal.*[1] From my observations I consider that I am entitled to conclude that nickel is visible in the solar atmosphere; I do not, however, yet express an opinion as to the presence of cobalt.

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[1] *Researches on the Solar Spectrum.* Roscoe's translation, part i. p. 20.

"Barium, copper, and zinc appear to be present in the solar atmosphere, but only in small quantities; the brightest of the lines of these metals correspond to distinct lines in the solar spectrum, but the weaker lines are not noticeable. The remaining metals which I have examined, viz., gold, silver, mercury, aluminium, cadmium, tin, lead, antimony, arsenic, strontium, and lithium, are, according to my observations, not visible in the solar atmosphere.

"Through the kindness of M. Grandeau of Paris, I obtained several pieces of fused silicium, and I was thus enabled, by using them as electrodes, to examine the spectrum of this element. The lines in the silicium spectrum are, however, with the exception of two broad green bands at 1810 and 1830, so deficient in luminosity, that

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[1] The italics are mine.

I was unable to determine their position with sufficient accuracy to reproduce them in my drawing. The two bright green bands do not correspond to dark bands in the solar spectrum, so that, as far as I have been able to determine, silicium is not visible in the solar atmosphere."

Confining his observations to the region of the solar spectrum between F and D, Kirchhoff found the following coincidences between lines in the spectra of certain elements and the Fraunhofer lines:—

	Lines.		Lines.
Sodium	2	Iron	42
Calcium	13	Chromium	4
Barium	7	Nickel	28
Strontium	2	Cobalt	10
Magnesium	3	Zinc	2
Copper	3	Gold	1

It will be seen from the foregoing that Kirchhoff deals mainly with the brightest lines, although the test failed him in the case of cobalt, for a reason I shall show further on. Hence, as a result of Kirchhoff's work, we had in the solar atmosphere—

Present.	Doubtful.	Absent.
Sodium.	Cobalt.	Gold.
Iron.		Silver.
Calcium.		Mercury.
Magnesium.		Aluminium.
Nickel.		Cadmium.
Barium.		Tin.
Copper.		Lead.
Zinc.		Antimony.
Chromium.		Arsenic.
		Strontium.
		Lithium.
		Silicium.

Herr K. Hofmann,[1] his assistant, continued these researches on both sides of the region observed by Kirchhoff, that is as far as A on one side and G on the other, and in addition he investigated the spectra of the following metals:—Potassium, rubidium, lithium, cerium, lanthanum, didymium, platinum, palladium, and an alloy of iridium and ruthenium. Hofmann added the following coincidences between lines of the spectra of these chemical elements and the dark solar lines:—

	Lines.		Lines.
Calcium . . .	16	Chromium . .	0
Barium . . .	5	Nickel	4
Strontium . . .	2	Cobalt	4
Magnesium . .	0	Zinc	3
Copper	1	Cadmium . . .	2
Iron	31	Gold	1

The spectra of the additional metals examined gave the following coincidences:—

	Lines.		Lines.
Cerium	2	Palladium	2
Didymium . .	2	Platinum.	1
Lanthanum . .	1	Rubidium and Iridium	1

The potassium spectrum could not be obtained by moistening the electrodes with salts of this metal, and when poles of the metal were employed the spectrum was so very feeble that only two prisms could be employed, and hence the position of the lines with regard to the solar lines was not easily determined. He noted that the line Kα was better seen if the Bunsen flame was used instead of the electric spark.

Kirchhoff and Hofmann pointed out how the latter work confirmed the results of the previous examination. A large number of lines of iron and of calcium occur in the yellow and the blue, and all these were found coincident with well-defined Fraunhofer lines. The probability that nickel was present in the solar atmosphere was greatly increased by the number of new coincidences observed. Cobalt remained doubtful, the solar lines coincident with a considerable number of its bright lines not having been observed. New coincidences in the spectra of barium, copper, and zinc with dark solar lines confirmed the presence of those elements in the sun's atmosphere. In the cases of strontium and cadmium the number of coincidences seemed to be too small to warrant the conclusion that those metals are in the sun. The other chemical elements examined, including potassium, did not appear to be traceable in the solar atmosphere. The case of potassium however they considered as doubtful, since faint solar lines are very near the red potassium lines.

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[1] Kirchhoff's *Researches*, translated by Roscoe, part ii., Appendix.

It is not too early to note that when we pass from the spectrum of the spark to the spectrum of the sun we are landed in doubt in many instances.

These then were the facts accumulated by Kirchhoff's method of solar observation, by which the spectra of the various solar regions were not separately observed.

Kirchhoff next discussed the bearing of this work on the physical and the chemical condition of the atmosphere of the sun. He says :—[1]

> " In order to explain the occurrence of the dark lines in the solar spectrum, we must suppose that the solar atmosphere incloses a luminous nucleus, producing a continuous spectrum, the brightness of which exceeds a certain limit. The most probable supposition which can be made respecting the sun's constitution is, that it consists of a solid or liquid nucleus, heated to a temperature of the brightest whiteness, surrounded by an atmosphere of somewhat lower temperature."

Of course this at once destroyed, at a blow, the idea of Sir William Herschel that the sun was a cool habitable globe, with trees, and flowers, and everything such as we know of here. If the atmosphere were in a state of sufficient incandescence to give these phenomena it was absolutely impossible that anything below that atmosphere should not be at the same time at a higher temperature. He says,[1] "The height of the solar atmosphere, judging from the phenomena observed in a total eclipse of the sun, is not small in comparison with the radius of that body; and hence the distances which two rays have to pass, one of which proceeds from the centre, and the other from the edge of the sun's disk, do not greatly differ." That was a reply to an objection which had been urged to the effect that if a dark line had been produced by anything absorbing in the atmosphere of the sun, there would be a very considerable difference between the spectrum of the sun's limb and the spectrum of the sun's centre, for the same reason, *mutatis mutandis,* that the sun is white at noonday and reddish at sunset; for since our atmosphere is thin, the light passes through a greater stratum in the latter case than in the former. At the sun the light would have to do the same thing, and we should get, therefore, a greater darkening of the limb than is actually observed. His words are: "We must remember that the lowest layers of our terrestrial atmosphere, or those in which the distance traversed by the light increases most rapidly when it approaches most nearly the horizon, are those which by virtue of their density must exert the most powerful absorptive action. In the solar atmosphere, on the contrary, it is those layers which are elevated to a certain position above the solid crust of the sun which act most energetically in producing the dark lines; for the lower layers which possess a temperature but slightly different from that of the mass, effect but little alteration on the light."

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[1] *Researches on the Solar Spectrum and the Spectra of the Chemical Elements*, part i. p. 23.

His theory therefore places the region where this absorption takes place at a considerable elevation in the atmosphere of the sun. His notion was that the sun we see is what gives us the continuous spectrum the light of which is absorbed; that above that there is a haze differing in structure from it, and yet not competent to give us the absorption lines; that practically none of the absorption phenomena arise from that stratum, but that the absorption phenomena really take place above this very luminous region of haze. Such was Kirchhoff's view.

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[1] *Researches on the Solar Spectrum and the Spectra of the Chemical Elements,* part i. p. 22.

Kirchhoff's explanation of the solar spots was that they are clouds in the atmosphere of the sun. He shows that local diminutions of temperature there, as in our own atmosphere, must give rise to the formation of clouds.

> "When a solar cloud is formed, all the portions of the atmosphere lying above it will be cooled down, because a portion of the rays of heat which are emitted from the incandescent surface of the sun are cut off by the cloud."[1]

The result of this would be that the upper portions of the clouds would rapidly increase and become cooler. The temperature of the cloud then falling below the point of incandescence it would become opaque and form the dark nucleus of a spot. He further supposed that the interposition of this dark cloud would so cool the regions above it that a second cloud would be formed of less opacity than the first, because the density of the vapour at that elevation would be less. This second cloud would appear as a semi-opaque penumbra.

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[1] *Researches on the Solar Spectrum and the Spectra of the Chemical Elements*, part i. p. 26.

CHAPTER VII.

THE WORK OF ANGSTRÖM AND THALÈN.

We now pass on to the next step, the work actually carried on or inspired by another eminent man no longer amongst us, Ångström. He took up very nearly the same work as Kirchhoff did, and extended it in certain directions; but he did the work in a different way instrumentally. Instead of the electric *spark*, he made use of the *arc* produced by the passage of a constant current of electricity from a powerful battery, between poles of carbon, the lower one of which served as a receptacle or crucible for the metal or salt under examination. A form of lamp commonly employed for this purpose is shown in Fig. 33. The poles are kept at a constant height and constant distance from each other as they burn away, by clockwork controlled by an electro-magnet.

This very powerful light enabled Ångström to use very high dispersion. He was not content with the kind of scale which Kirchhoff had employed, a scale dependent on the construction of his instrument. He wished to have a natural scale. He therefore rejected prisms, and used a diffraction grating, devised, as we have seen, by Fraunhofer. By means of this he obtained what was called, and what is still called, a *normal spectrum;* and having obtained this, he, like Kirchhoff, endeavoured to determine the concidence, or want of coincidence, of metallic lines with those visible in the spectrum of the sun.

A word about the modern grating before we proceed. Fraunhofer's parallel wires were soon replaced by scratching fine lines on a plate of glass through which the light to be analysed was made to pass. It has since been found better to reflect the light than to transmit it.

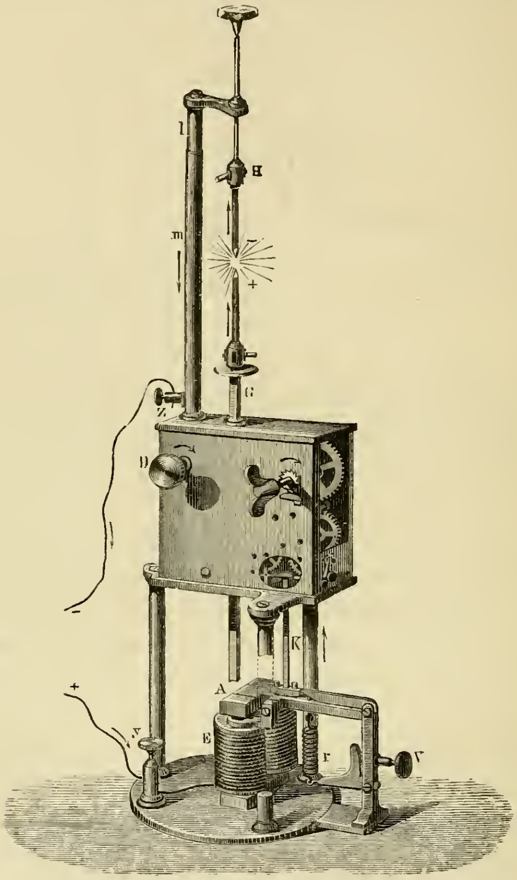

FIG. 33.—Electric lamp. Y, Z, wires connecting battery of fifty Grove or Bunsen elements; G, H, carbon holders; K, rod which stops a clockwork movement which, when going, makes the poles approach until the current passes; A, armature of a magnet, which by means of K frees the clockwork when not in contact; E, electro-magnet, around which the current passes when the poles are at a proper distance apart, causing it to attract the armature A.

A grating as now used is made by ruling, with a diamond and dividing machine, a great number of very fine, parallel, equidistant lines on a piece of highly polished speculum metal, or glass which is afterwards silvered on the scratched surface. The manufacture of such gratings is carried to great perfection, and some made by Mr. Rutherfurd contain as many as 17,000 lines to the inch.

When light falls normally on such a grating, and we obscure the light reflected from it, we find that a portion of it is reflected back unaltered, as it would be from an ordinary mirror, while on each side of this, at some distance from it, a spectrum is formed (called a spectrum of the first order). Outside this, again, on each side, at a greater angular distance from the normal, another spectrum appears (second order) of much greater dispersion than the first. This is succeeded by a spectrum of the third order, and this again by a fourth, and so on, each time with a greatly increased dispersion and decreased luminosity. The observing telescope can then be directed to the grating at such an angle as to enable either of these spectra to be observed as it would be in an ordinary spectroscope.

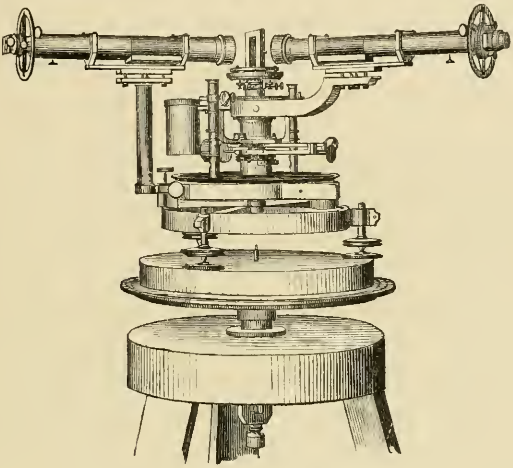

FIG. 34.—Ångström's spectrometer.

The advantage of the grating over the prism lies in the fact that it gives a purer spectrum, for the light has no absorptive material to pass through, such as prisms, the glass or surfaces of which may be imperfect.

But there is another and much more powerful reason which induced Ångström, Mascart, and many others who might be named, to employ diffraction in preference to refraction.

By the use of these diffraction gratings measured with great care and expressed in terms of the standard metre, along with a collimator and observing-telescope—the latter fitted with a micrometer screw which enables us to determine with great accuracy the angle through which it moves—we are able to determine, in the most convenient manner and with great exactness, the wave-length of the light we are observing. In this way Ångström determined the wave-length of the more prominent lines of the solar spectrum from A to H.[1] Using these lines as starting-points he was able, by means of the micrometer, to measure the angular distance between any of these points and any line which lay between them, and then, writing these determinations in interpolation formulæ, he was able to compute the wave-length of any observed solar line.

The wave-lengths of some of the principal Fraunhofer lines as given by Ångström are as follows :—

A	·00076009	E	·00052689
B	·00068668	F	·00048606
C	·00065618	G	·00043072
D¹	·00058950	H	·00039680
D″	·00058890	K	·00039328

---

[1] Knowing the velocity of light, the wave-length of any particular line can be calculated by the following formula :—

$$\lambda = d\,\frac{\sin\delta}{n}$$

when λ = the wave-length.
d = distance between lines of grating in millimetres.
δ = observed angle.
n = order of the spectrum.

These numbers show that the difference between the blue light at one end of the beautifully coloured band and the red at the other, is nothing more nor less than a difference almost identical with the difference between a high note and a low note upon the piano. The reason why one end of the spectrum is red and the other blue, is that in light as in sound we have a system of disturbances or waves; we have long waves and short waves, and what the low notes are to music the red waves are to light. The dispersion of light, whether effected by refraction or diffraction, is simply the sorting out, and arranging in regular succession, of the various light-tones in the order of their wave-lengths.

We can now recognise the strict analogy between the world of sound and the world of light. Ears are tuned to hear different sounds—some people can hear much higher notes than others, and some people can hear much lower notes than others. In the same way some people can see colours to which other people are blind; indeed, the more we go into this matter, and the more complete we make our inquiries, the more striking becomes the analogy between these two classes of phenomena.

The latest measurements tell us that a light-producing disturbance travels at the rate of 186,000 miles in a second of time. Imagine the molecular agitation depending upon this statement, and then remember that a glow-worm can set it all going, and that, when once in full swing, the distance of the most remote star is traversed, as it were, at a bound, and without sensible loss of energy.

Now as in 186,000 miles there are 298,000,000 metres, or 29,800,000,000,000,000,000 hundred-millionths of a millimetre, and as all the waves must enter the eye in a second, we have, for the number of wave crests per second,

$$\frac{29{,}800{,}000{,}000{,}000{,}000{,}000}{76009} = 392{,}000{,}000{,}000{,}000,$$

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[1] A millimetre is 0·03937 of an inch.

that is, 392 billions of waves entering our eye each second in the case of red light, taking the line A in the spectrum, and

$$\frac{29{,}800{,}000{,}000{,}000{,}000{,}000}{39{,}328} = 757{,}000{,}000{,}000{,}000$$

that is 757 billions in the case of violet light, taking K in the spectrum.

As the velocity of light is the same for all waves, it follows that the number of waves per second varies inversely as the wave-length in each case, and that the number of waves per second multiplied into the wave-length of any particular line must give us a constant quantity, namely, the velocity.

In Ångström's memoir[1] the wave-lengths are given to the second decimal place, the unit being $\frac{1}{10000000}$th of a millimetre.

In the Atlas which accompanies this memoir the scale is divided so that one division corresponds to $\frac{1}{100000000}$th of a millimetre of wave-length. In addition to marking the wave-lengths of the solar lines, their relative intensities are shown. The map also gives the origin of each line and its correspondence with the lines of metallic spectra so far as these have been determined by Ångström and Thalèn.

The first results[2] of Ångström's comparison of the wave-lengths of metallic vapours with the Fraunhofer lines added the possibility of strontium and aluminium being among the solar elements. He thus allocated the principal Fraunhofer lines :—

H^1 and H^2 to Calcium.
G ,, Iron
F ,, Strontium and Iron (uncertain).
b ,, Magnesium and Iron.
D ,, Sodium.
C ,, Hydrogen.
B ,, Potassium.

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[1] *Recherches sur le Spectre Solaire*, Upsala, 1869.

[2] Communicated to the Royal Academy of Stockholm, October 8, 1861. *Phil. Mag.* vol. xxiv. s. 4, p. 1.

The following is a summary of the coincidences observed:—

Sodium . . .	9 (all)	Magnesium . .	4 (3 ?)
Iron	450	Chromium . .	18
Calcium . .	75	Nickel	33
Cobalt . . .	19	Hydrogen . . .	4 (all)
Manganese .	57	Titanium . . .	118
Barium . . .	11 (of 26)	Zinc	2 ? (of 27)
Aluminium .	2 ? (of 14)	Copper	17

Ångström remarks that the number of these lines, about 800, might easily be increased by raising the metals to a higher stage of incandescence. Still, he observes, the number already found is quite sufficient to enable him to refer the origin of almost all the stronger lines of the solar spectrum to known elements, thus confirming the opinion he had expressed in a previous memoir, that the substances which constitute the mass of the sun are doubtless the same as those forming that of the earth. But, he writes, the fact must not be lost sight of that there exist nearly midway between F and G strong solar lines, of which the origin is entirely unknown.

Ångström[1] gives no list of elements present in the sun such as that given by Kirchhoff, but in its place the table of coincidences printed above. Thalèn, his associate, in a separate memoir,[2] gives, however, as present in the sun—

Sodium,
Iron,
Calcium,
Magnesium,
Chromium,
Nickel,
Cobalt,
Hydrogen,
Manganese
Titanium,

thus rejecting zinc, barium, and copper from Kirchhoff's list of accepted elements, adding cobalt from the doubtful list, and hydrogen and manganese from Ångström's, and titanium from his own observations.

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[1] *Recherches sur le Spectre Solaire*, par A. J. Ångström. *Spectre Normal du Soleil.* Berlin, 1869.

[2] *Longueurs d'Onde des Raies Métalliques*, p. 11. Nova acta. Upsala. 1868.

The table of coincidences referred to, and Ångström's remarks thereon, explain the cause of this. Kirchhoff's evidence for zinc had depended upon the coincidence of two lines only, and these were doubtless thought insufficient, as in the cases of the metals retained in the list the number of the coincidences was much greater.

From Ångström's remarks, which I proceed to give, it is evident that he was not quite satisfied with the brilliancy test relied on by Kirchhoff, and that his doubts concerning zinc arose from this cause.

Of aluminium he writes [1] that although it gives brilliant lines in different parts of the spectrum, yet the two lines situated between Fraunhofer's two H-lines are the only ones which appear to coincide with solar ones. By way of explanation of this phenomenon he points out that the violet rays are much the strongest in the spectrum of this metal. He observes that these two lines often present the same phenomenon of absorption as is shown by the yellow sodium lines, which is a proof of their great intensity. He states finally that the point will be cleared up by ascertaining whether the ultra-violet lines of aluminium coincide or not with faint solar lines in that region.

Of zinc he remarks [2] that the two lines he has given of that metal as coincident with solar lines do not correspond with the latter in character, being wide, very strong and nebulous, so that the presence of zinc in the sun remains doubtful. It is noteworthy, however, that there are three lines in the magnesium spectrum which present the same nebulous appearance, and to which there are no corresponding solar lines, and yet magnesium, he thinks, is undoubtedly present in the sun.

Ångström intended to supplement his normal solar spectrum with a series of maps of the bright lines of all the metallic elements on a wave-length scale, but circumstances prevented his doing so, and at his request Thalèn, his associate, undertook their preparation.[1]

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[1] *Loc. cit.* p. 36.

[2] *Idem.*

Thalèn's method of work was to compare the metallic lines with the solar lines, and his wave-lengths were then taken from Ångström's *Spectre Normal.*

He, like Kirchhoff, employed a large Ruhmkorff coil and Leyden jar, and took the spark between electrodes of the metal under observation, or of platinum or aluminium moistened with solutions of salts of the various metals. The salts employed were usually the chlorides, on account of their ready volatility. Occasionally, instead of the spark, an arc produced by fifty cells was used.

The dispersion generally employed was afforded by a single prism of bisulphide of carbon, and was magnified by a very powerful telescope. Sometimes a second similar prism was added, and on rare occasions these were replaced by six prisms of dense flint glass. Occasionally also, when low dispersion was required, a single prism of flint glass was used.

In some cases when the metallic lines were very faint, the direct method of solar comparison could not be applied, because the glare of the sunlight, even when reduced by a diaphragm, was so great by comparison that the feeble metallic lines could not be discerned. Another method had, therefore, to be resorted to. Instead of producing the solar and metallic spectra side by side, they were superposed. The sunlight being then cut off, a pointer was brought into exact coincidence with the line observed, and then the sunlight was again allowed to enter, and the position of the pointer among the Fraunhofer lines gave the position of the metallic line.

Thalèn gives only the strong lines of the metals in his maps because he deemed it impossible to prove that the feeble lines were not due to some other element present as an impurity, but he remarks that the spectroscope itself should be capable of settling this question of impurities—an opinion which we shall see subsequently has been in part at least verified.

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[1] *Mémoire sur la Détermination des Longueurs d'Onde des Raies Métalliques.* Upsala, 1868.

The difficulty Thalèn experienced in this question of impurities will be seen from his remarks on titanium. Concerning this element, he states that the faintness and transitory character of its lines rendered it difficult to determine any solar coincidences, but he nevertheless found some such coincidences in the green.

Subsequently Ångström and Thalèn observed these lines in the electric arc taken between poles of charcoal soaked in a solution of calcic chloride, but they were not seen in the spark between metallic electrodes moistened with a solution of calcic chloride.

Thalèn then prepared some chemically pure bichloride of titanium, and satisfied himself spectroscopically, by the absence of the calcium lines, that it contained no trace of calcium, while from the presence of the green lines above mentioned he concluded that those lines are certainly due to titanium and not to calcium; and, further, that the carbons employed as electrodes in the voltaic arc contained an impurity of titanium.

From the number of fine lines of titanium concident with solar lines Thalèn concluded the existence of titanium in the sun.

In the case of iron and other bodies he notes that there is an agreement *to some extent* between the intensities of the solar and metallic lines, but that in the case of titanium this is less apparent.

Kirchhoff's and Ångström's maps are in all our laboratories, and there is a very considerable difference between them. This difference arises from the fact that whereas Kirchhoff used an induction coil and spark, Ångström varied his experimental method by placing no longer a spark, but the electric arc in front of the slit of his instrument. In this case, therefore, he was determining the spectrum which was produced at the temperature of the electric arc instead of the spectrum which was produced at the temperature of the electric spark.

The result of the combined attack of Kirchhoff, Ångström, and Thalèn is shown in the accompanying table:—

Elements present in the Sun.

Kirchhoff.	Ångström and Thalèn.
Sodium.	Sodium.
Iron.	Iron.
Calcium.	Calcium.
Magnesium.	Magnesium.
Nickel.	Nickel.
Barium.	—
Copper.	—
Zinc.	—
	Chromium.
	Cobalt.
	Hydrogen.
	Manganese.
	Titanium.

So far then for that mode of observing the sun which consists in comparing the general light of the light-source with the general light of the sun.

This introduces an important consideration. When we have a light-source placed in front of the slit of the spectroscope it is perfectly clear that light from all portions of the light-source must illuminate the slit. Similarly, if we content ourselves by pointing the spectroscope to the sun, or to a cloud illuminated by the sun, it is perfectly obvious that the light from all parts of the sun must enter all parts of the slit, and if there is any localisation of phenomena on the sun this will be lost to a greater or less extent in the spectroscopic record. Such localisations there undoubtedly are, as may be seen with a telescope of very moderate power, and a brief account of what these are, and how they may be effectively got at by the spectroscope, may be conveniently given in the next chapter.

CHAPTER VIII.

A NEW METHOD OF WORK.

1. *Details of the Solar Surface.*

We must now pass to details of another order. Those we have given refer to the light of the sun taken as a whole. But it is in the highest degree necessary for the purposes of our present inquiry, that we should not content ourselves with this general view—since when we pass from Kirchhoff's work we pass to work done on minute portions of the solar surface. This we must now consider, and it is desirable that I should preface it as briefly as may be by a reference to the various differences observed in different parts of the sun.

When we look at the sun with a powerful telescope, taking all needful precautions (never look at the sun with a small telescope, for this is a most dangerous proceeding), we find it to be by no means the immaculate body it was thought to be by the schoolmen. Here and there on the disc, but generally limited to those parts of it a little above and below the equator, dark spots may be observed. These generally exhibit three shades of darkness, and float, as it were, on the general bright surface of the sun called the photosphere, the darkness of the spot increasing towards the apparent centre. We have first the *penumbra,* then the *umbra,* then the *nucleus.* But sometimes the darker portions are excentric, and very irregular in outline.

Changes are going on incessantly in the region of the spots. Sometimes these are noticed after the lapse of an hour even; a portion of the penumbra setting sail across the umbra; or a portion of the umbra melting from sight; or we may have an evident change of position and direction in masses which retain their form. In some spots evidences of cyclonic action are very obvious.

We not only get this darkness localised in spots, but it is easy to see that there is a general defect of illumination as we approach the sun's edge.

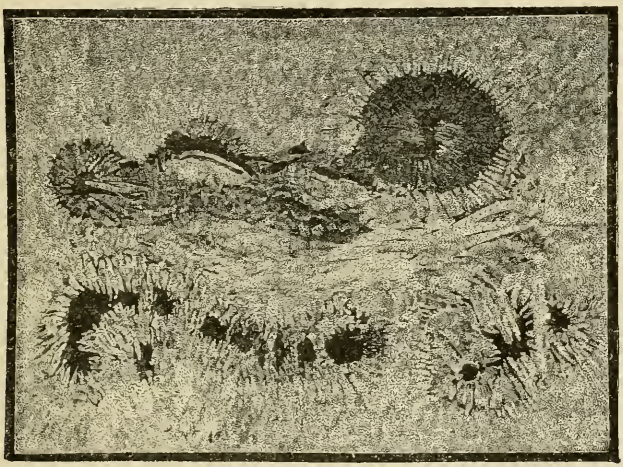

FIG. 35.—Sun spot (Secchi).

We may now pass to the brighter portions of the general surface. These are most obvious near the edge of the solar disc, and especially about spots approaching the edge; in these positions bright streaks of diversified form, quite distinct in outline, and either entirely separate or uniting in various ways into ridges and network, are seen. These, which have been termed *faculæ*, are the most brilliant parts of the sun. Where near the edge the spots become invisible, undulated shining ridges still indicate their place, being more remarkable thereabout than elsewhere, though everywhere traceable in good observing weather. Faculæ may be of all magnitudes, from hardly visible softly gleaming narrow tracks, 1,000 miles long, to continuous complicated and heapy ridges 40,000 miles and more in length, and 1,000 to 4,000 miles broad. Ridges of this kind often surround a spot, and hence appear the more conspicuous; but sometimes there appears a very broad white platform round the spot, and from this the white crumpled ridges pass in various directions.

So far we have referred only to the phenomena at all times visible to us with ordinary telescopic aid, but those who have been favoured by a sight of a total eclipse, and many more who have read the accounts of total eclipses, know that there is a great deal more of the sun than one generally sees. On these occasions a part of the sun not usually visible—its external atmosphere—is unveiled for us. The central light of the sun being cut off by the intervening dark moon, surrounding it, on all sides, appears a glorious halo, generally of a silver-white light; this is called the corona; now radiated in structure, and again full of strangely-curved markings or long streamers, it extends sometimes beyond the moon to a distance equal to many diameters.

Recent eclipse work has shown that the corona, in part, at all events, reveals to us the sun's outer atmosphere, which is invisible when the sun's light itself is present, owing to the overpowering light of the latter. But the corona is not all we see at such times.

When the totality has commenced, apparently close to the edge of the moon, and therefore within the corona, are observed fantastically-shaped masses generally full lake-red, fading into rose-pink in colour, variously called red flames or red prominences.

The height of some of these prominences exceeds 70,000 miles. They were first described by Stannyan in 1706. Since his time many have held them to be beautiful effects produced by the passage of the moon over the sun, or even clouds in the atmosphere of the moon coloured, by the strange way in which the solar light then fell upon them, but their solar origin has now been conclusively established, chiefly by the observations of the eclipses of 1842, 1851, and 1860.

The observations of the eclipse of 1842 afforded evidence that these red prominences or flames—these different-coloured phenomena—were really, so to speak, upper crests of an almost continuous cloud-sea round the sun.

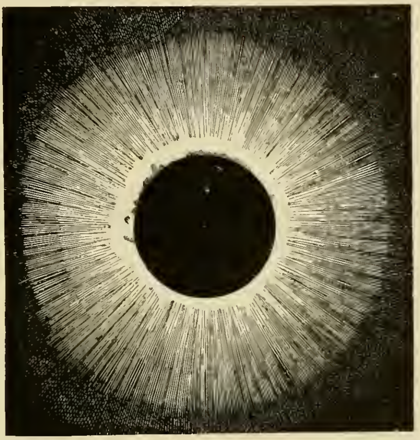

FIG. 36.—Total eclipse (Dawes), 1851.

In the drawings in question, a fine low level, of the same colour as the prominence itself, was shown connecting the prominences, while later drawings gave us those prominences alone after the moon had covered all the lower portion. That any doubt should have remained after such an observation as this is as good an indication as I can give of the extreme difficulty of making observations during eclipses, and how important it is that one should have a method which makes us independent of them.

The above hasty sketch of localised solar phenomena must suffice for the present; in the sequel we shall have to return to the various points from the chemical point of view.

2. *How these Details can be Spectroscopically Observed.*

Now it is evident that if we simply direct a spectroscope to the sun, or reflect sun-light into it, it will receive light coming indiscriminately from spots, faculæ, and general surface, and if there are any differences in the spectra of these regions, in other words, if the chemistry of the various regions is different, the differences will be lost, since we shall obtain a mixed spectrum partly due to the spots, partly to the faculæ, and partly to the general surface.

But there is another way of observing the spectrum of the sun. *We can throw an image of the sun, or of any part of the sun, on the slit of the spectroscope.* This kind of work, though, as we have seen, it was suggested by Forbes thirty years earlier, and actually employed by Ångström to investigate the darkening of the limb, was first applied to the spots and faculæ in 1866.

If a spot or a facula be visible on the sun, we can throw the image of the sun on the slit plate, and then bring it exactly on the slit. We shall in this way get the spectrum of the sun-spot, or facula, as distinguished from the spectrum of the other portions of the sun.

The manner in which this kind of work is carried on is easily grasped. It simply consists in the use of a spectroscope of large dispersion attached at the focal point of a telescope of considerable power.

Fig. 37 shows the eyepiece end of Mr. Newall's giant refractor, with a spectroscope with a considerable number of prisms, fixed to the telescope by means of an iron bar. The slit of the spectroscope occupies the focus, so that when the instrument is pointed towards the sun we see an image—in the case of this telescope something like four inches in diameter—with the spots and brighter portions wonderfully and beautifully clear; and by means of the different adjustments of the telescope we can bring now a spot, and now one of the brighter portions of the sun, on to the slit. If there be any difference between the spectrum of the spot and the spectrum of the general surface of the sun, such difference will be thus observed at once, and the differences are in fact very striking

Such an instrument, compounded of the telescope and spectroscope, has been named a telespectroscope.

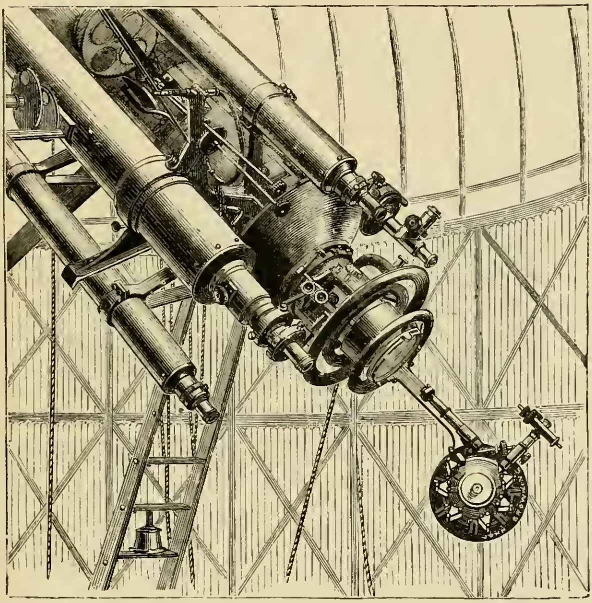

FIG. 37.—The eyepiece end of the Newall refractor (of 25 inches aperture) with spectroscope attached.

The next point to engage our attention is the change in the phenomena produced by this change in the method of observation. We will first deal with spots.

3. *Spot Spectra.*

When an image of a sun-spot has been thrown on the slit of the spectroscope in the manner described, it shows itself in the spectrum as a dark band, of greater or less width according to the size of the spot, running along the whole length of the spectrum, and crossed, as are the other parts of the spectrum, by the Fraunhofer lines; the nucleus is seen as a narrow band darker than the rest. Besides this continuous absorption, another phenomenon is seen. Some (often many) of the Fraunhofer lines in the parts where they are crossed by the spot spectrum, are considerably thickened, the widening being greatest in the nucleus and gradually fading away in the penumbra.

In Fig. 38 this appearance is well depicted. The spectroscope has been so placed that its slit bisects two spots, in each of which many lines are seen widened, notably the two D lines of sodium. It is not always the same set of lines that are widened. In some spots the sodium lines are most affected, in others the iron lines, and so on.

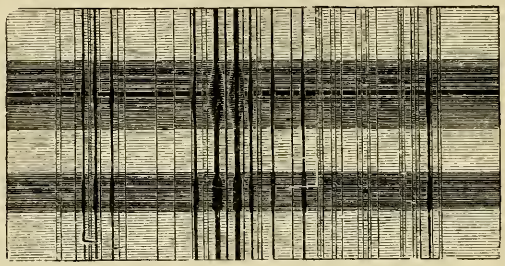

FIG. 38.—Spectrum of sun-spot, showing the widening of the D lines.

I was induced to start this method of observation in 1866 for the following reason:—

Two rival theories had been suggested to explain how it was that a sun-spot is dark.[1] One school, led by M. Faye, believed that the interior of the sun is a nebulous gaseous mass of feeble radiating power at a temperature of dissociation, and that surrounding this is a highly radiating photosphere. On this hypothesis, in a sun-spot, the nebulous interior mass is revealed through an opening in the photosphere, caused by an upward current, and the spot is black by reason of the feeble radiating power of the nebulous mass. The other school, comprising Messrs. De La Rue, Stewart, and Loewy, referred the appearances connected with sun-spots to the effects, cooling and absorptive, of an inrush or descending current of the sun's external atmosphere, which must be colder than the subjacent photosphere.

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[1] See *Proc: Roy. Soc.* 1866, vol. xv. p. 256.

By some observations communicated to the Royal Astronomical Society[1] in 1865, I was led independently to this latter conclusion. The observations indicated that instead of a spot being caused by an upward current, it is caused by a downward one, and that the results, or, at all events, the concomitants of the downward current, are a dimming and possible vaporisation of the cloud masses carried down. I was led to hold that the current had a downward direction by the fact that one of the cloud-masses observed, passed in succession, in the space of about two hours, through the various orders of brightness exhibited by faculæ, general surface, and penumbræ.

If we had been dealing with *defective* radiation, we should still have been dealing with radiation, and should have expected to see bright lines; but no obvious bright lines were seen in the spectrum of the spot; what we did see was the thickening and darkening of certain lines and the continuous absorption. In the case of the lines of sodium it was very marked; so that we were perfectly justified in saying that the sun-spot was really not produced by any defect of radiation, but was truly and really produced by an increased amount of absorption.

4. *Spectrum of the Limb.*

The darkness of the spectrum at the limb is very different from the darkness in a sun-spot.

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[1] *Monthly Notices Roy. Ast. Soc.* vol. xxv. p. 237.

Ångström, in extension of Forbes's observation of the spectrum of the limb of the sun during an annular eclipse, repeated the observation under conditions which were more likely to reveal any differences, if such existed, between the spectra of the centre and limb. He projected an image of the sun on the slit of the spectroscope, and then at his leisure was able to see whether the Fraunhofer lines underwent any change when different parts of the image were allowed to fall on the opening. The results of this observation are thus described by Ångström :—[1]

> "Any very remarkable change I could not discover; all that I fancied I could remark was, that the intensity of the spectrum light is somewhat less when the ray comes from the edge, than when from the centre of the disc; and this is evidenced by the circumstance that the fainter Fraunhofer lines show themselves in the latter case comparatively stronger, whereas when the light comes from the centre of the solar disc, the fainter lines will sometimes even totally disappear, while the stronger lines, as for example, some of the iron lines, appear with correspondingly increased brilliancy; as we know by Kirchhoff's experiments that an increased difference of intensity between the source of light and the absorbing gas is favourable to the distinctness of the lines in the gas spectrum, it would seem that this observation, if confirmed, is not repugnant to what we already know concerning the absorbing power of gases."

To facilitate the comparison of the spectrum of the limb with that of the centre of the sun, I used a specially-constructed slit plate. One half of the slit is covered by a totally reflecting prism, and another similar prism actuated by a fine screw is fixed in a slide, so that its distance from the former can be varied. The centre of the sun's image is allowed to fall on the uncovered part of the slit, and its edge on the sliding prism, from which it is reflected to the fixed prism, and thence into the collimator, which it enters side by side with the direct light from the centre of the disc.

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[1] *Phil. Mag.* s. 4, vol. xxiv., July, 1862, p. 3, *note.*

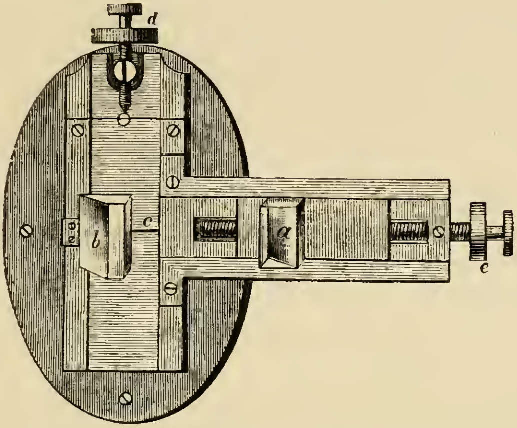

Fig. 39.—*a*, right-angled prism on which the image of the limb falls ; *b*, right-angled prism which receives light from *a* ; *c*, slit through which centre of sun is observed ; *d*, screw to adjust width of slit ; *c*, screw to adjust distance of *a* from *b*, according to the semi-diameter of solar image employed.

An arrangement suggested by Mr. Hastings for accomplishing the same result is shown in Fig. 40. The light from the edge of the sun reaches the slit after two total reflections from the faces of the prism, as shown in the figure, while that from the centre enters the uncovered part of the slit directly.

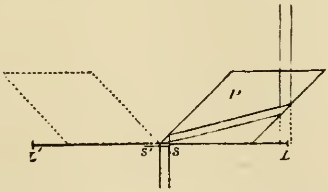

Fig. 40.—Mr. Hastings's arrangement for comparison of spectra. *s s'*, slit ; *l l'*, diameter of sun's image ; *p*, totally reflecting prism.

In spite, however, of the new apparatus, it was very difficult to see any special effect at the limb.

An increased selective absorption at the limb appeared especially probable for the violet region, because the difference between centre and limb is more marked photographically than visually, a view which received support from Herr Vogel's observations.[1] With minute search however, Mr. Hastings[2] obtained evidence of increased selective absorption not only in the extreme violet, but also in the visible part of the spectrum.

He found that the lines b_1, b_2, and b_3, "become sharper and less hazy near the limb;" C and F also become sharper in the same region, and D is affected; while on the other hand a line 768·1 (Kirchhoff), which is strongly marked in the centre of the disc, disappears entirely within 16″ to 20″ from the limb. Two other lines near F (1828·6 and 1830·9 Kirchhoff) which are strong at the limb become much fainter at the centre. "These latter lines also become greatly strengthened over the penumbræ of spots," while "768·1 is not thus affected."

Besides these differences which were constantly observed, others were suspected, and Mr. Hastings thus discussed the phenomena:—

> "Since the light from the border of the sun undergoes a general absorption, which reduces its intensity to much less than one-fourth that at the centre, according to Secchi's measurements, and yet the spectroscopic character is changed so slightly, it is impossible for me to escape the conviction that the seat of the selective absorption, which produces the Fraunhofer lines, is below the envelope which exerts the general absorption. But the phenomena of the faculæ prove not only that this envelope rests upon the photosphere, but also that it is very thin. The origin of the Fraunhofer lines, then, must be in the photosphere itself, which is in accordance with Lockyer's views.
>
> "Any effects which the chromosphere might produce, we would anticipate finding most evident in the lines of those gases which are readily detected there. A reference to the observations shows at

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[1] *Naturforscher*, Jahrgang v. p. 321, Oct. 1872.

[2] *Nature*, vol. viii. p. 77, May 22, 1873.

once a compliance with this anticipation in the lines of hydrogen, magnesium, and sodium. The line 768·1 is not less strikingly in concordance, if it be regarded as 768·?* (the ? indicates doubt as to the tenths of the scale, and * absence of a corresponding black line) of Young's catalogue of chromospheric lines. The lines 1828·6 and 1830·9, with others of the same class, probably have their origin in the medium which exerts the general absorption, and thus are allied to our telluric lines. It also seems probable that the chromosphere is too transparent to reverse many of its lines. That this is the case in the helium lines is tolerably certain."

5. *Spectrum of the Faculæ.*

When a facula is brought on to the slit of the spectroscope, it manifests its presence by a band of greater brightness than elsewhere running along the whole length of the spectrum. We have in fact an indication of less absorption, general as evidenced by the greater brightness of the continuous spectrum, and selective as shown by the thinning and even disappearance of some of the dark lines.

This and other observations have led to the conclusion that the faculæ are elevated above the general surface as the spots are depressed,—that they are in fact the higher cloud-domes of the photosphere. This will at once explain why the faculæ are best seen near the sun's limb. The difference between the general absorption of a facula and that of the general surface of the sun is so small, that it may be scarcely perceptible on the centre of the disc, but becomes very apparent at the limb, where a greater thickness of atmosphere is brought into play.

It is seen then that the new method actually does give us important spectroscopic differences, not only between the spectra of spots and faculæ, but between the spectra of both spots and faculæ as contrasted with that of the general surface.

CHAPTER IX.

MORE RESULTS OF THE NEW METHOD.

1. *Artificial Eclipses.*

ALTHOUGH in 1866 much knowledge had been gathered concerning the strange red prominences seen round the edge of the moon in a total eclipse of the sun, they were still a mystery and a source of wonder. After the spectroscope had been used to help us in the study of the spots, it seemed worth while to inquire whether it could also help us in other directions, for if it could it was perfectly clear that we should not be contented with merely observing the chemical nature of the photosphere alone.

It did not take long to convince my friend Dr. Balfour Stewart and myself, that by means of the spectroscope, the things which up to that time had only been observed during eclipses, would be more or less *felt*, if they were not absolutely rendered visible, by this new instrument, if their real nature was anything like what we were justified by our then knowledge in attributing to them ; and for this reason : the things seen round the sun during an eclipse are not there for the instant of the eclipse only : they are always there : why then do we not see them ? The illumination of our own air prevents this. What is our own air illuminated by ? By the sunlight. Now whereas increasing dispersion does considerably dim a continuous spectrum for the reason that it makes it extend over a much larger area, it does not dim to any great extent the brightness of a line ; so that by employing a considerable number of prisms we ought to be able to abolish the illumination of our air altogether, and in that way we should no longer be limited to determining merely the chemical nature of the spots, we should be equally able to determine the nature of the surrounding solar atmosphere, supposing the phenomena observed during eclipses were really due to incandescent vapours at the sun, and were not lunar or terrestrial.

This principle can be very clearly demonstrated by means of the electric light. The lower pole is charged with some metallic salt, say a salt of lithium, and a single prism is interposed in the beam. There then appears on the screen a mixed spectrum, due partly to the continuous light from the solid poles, and partly to the lithium vapour. The red lithium line stands out on a background of continuous spectrum. On mounting a second prism, the continuous spectrum from the poles being much more dispersed is enfeebled, while the bright line of lithium retains almost its original brilliancy because it is *not* dispersed. On the addition of a third prism this effect is enhanced. In fact, the brightness of the *line* relatively to that of the *continuous spectrum* increases so rapidly with each increase of dispersion, that by employing a sufficient number of prisms, we can practically abolish the latter and see the bright line on a dark ground. That was the principle which it was suggested would enable the spectroscope to be used in making what have been called *artificial eclipses.*[1]

This method was first applied by Janssen immediately after the eclipse of 1868.

Now if we consider what are the conditions presented by eclipses, as well as the sort of thing the spectroscope is called upon to observe, we shall see the very considerable advantage of the introduction of the new method. In the first place, eclipses, which are so full of teachings to be got only at the moment of their rare occurrence, are almost instantaneous, so far as each particular phenomenon is concerned; and, secondly, when the duration is, say, four or five or six minutes, which happens but very rarely, although a great deal of work may be done, only a very small part of the more interesting regions of the solar atmosphere is uncovered.

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[1] *Proc. Roy. Soc.* 1866, vol. xv. p. 258.

How then can we avail ourselves of this method? It should be perfectly clear that if instead of observing a spot we allow the slit to lie on the edge of the sun, and then sweep round it, if the method is competent to abolish the illumination of our atmosphere—to make the bright lines visible—that here and there if the slit travels over a prominence it will admit the light of the prominence; and if we have the image of the sun very accurately focused on the slit, the size of the image of the sun and the length of our slit being known, the length of the slit illuminated by the prominence will enable us readily to determine the exact height of the prominence. Further, if it should happen that there is a sort of vaporous sea round the sun usually invisible, but which this new method will reveal, it would follow that we shall get the depth of the sea sounded for us by the length of the line.

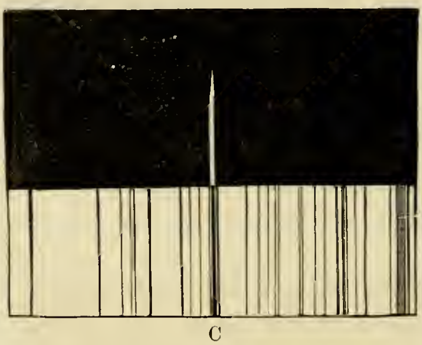

FIG. 41.—Line C (red), with radial slit.

Again, if we do sweep round the sun in this way, and if these prominences really do give us lines, we have exactly the same method of determining the chemical nature of this exterior atmosphere as Kirchhoff employed in determining the composition of the general light of the sun. Only we have a great advantage in this case, for whereas Kirchhoff had to suggest an hypothesis to explain the possible locus of the region which produced the lines due to the different chemical substances, we have the hard fact beneath our eyes, because if we encounter a prominence, and if it be built up, let us say, of iron vapour, then we shall see iron lines; if it be built up of calcium vapour, then we shall see calcium lines, and so on. Now what are the facts? The first observation that was recorded with absolute certainty

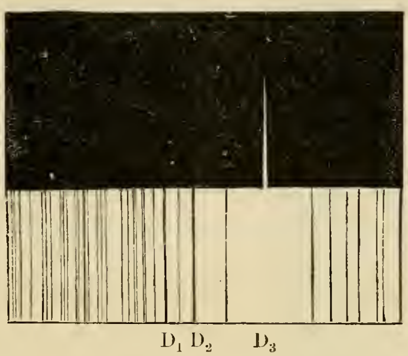

FIG. 42.—Line D_3 (yellow), with radial slit.

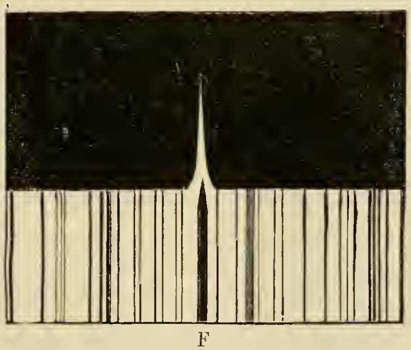

FIG. 43.—Line F (blue-green), with radial slit.

touching the chemical nature of the exterior envelope of the sun had reference to the red line of hydrogen. When therefore we had such an observation as this, showing one of the lines produced by this vaporous sea, coincident with the C line of the solar spectrum, we knew at once, on the assumption made by Kirchhoff, that that line was produced by hydrogen. It was necessary, of course, that the other lines of hydrogen should be investigated. The next obvious line of hydrogen is F in the blue-green, and when the question was put to this line, in that case also it was found that the prominences gave out no uncertain sound—that the prominences were really and truly composed to a large extent of what we call hydrogen; that is to say, the spectral lines observed when we render hydrogen incandescent are identical with three of the spectral lines observed when we throw one of the solar prominences on the slit. Here then we see that as in the case of the spots we are in full presence of localised chemical phenomena.

All the prominences have not this simple constitution. Some of them exhibit, besides the lines of hydrogen, those of magnesium, sodium, iron, and other metals; indeed they may be divided according to their chemical composition into prominences in which among known lines those of only hydrogen are seen, and others in which the lines visible in the spectra of a great number of metals are very brilliant.

Extended spectroscopic observation outside the sun's limb on this new method not only revealed the chemical nature of the prominences, but proved that *they were merely local heapings up of an envelope chiefly characterised by the hydrogen lines which entirely surrounded the sun.* It was found that outside the photosphere the prominence spectrum was never absent. To the continuous envelope thus revealed I gave the name *Chromosphere* —a term suggested by my late friend, Dr. Sharpey, who was then one of the secretaries of the Royal Society, and who took the keenest interest in the new revelations—to distinguish it from the fainter outer atmosphere as seen in eclipses on the one hand, and the white photosphere on the other.

It was soon found that this continuous ocean, this continuous outer shell of the sun, varied considerably in thickness from time to time, and it was also found that the lines seen in the spectra of other substances besides hydrogen, some of which at present we know nothing of, others of which we now think we know a great deal of, also appeared side by side with the lines of hydrogen. Sometimes the spectrum of the chromosphere is full of lines. In almost all cases, however, we find that these lines are never so long as the hydrogen line, from which we gather that the depth of the layer of the solar atmosphere measured for us by the length of the magnesium lines, to take a case, is much less than that of the hydrogen one. I should further add here that when the sun is moderately active and can be well observed, as in a fine climate like that in Italy, this magnesium layer can be detected all round the sun, so that we have in the chromospheric

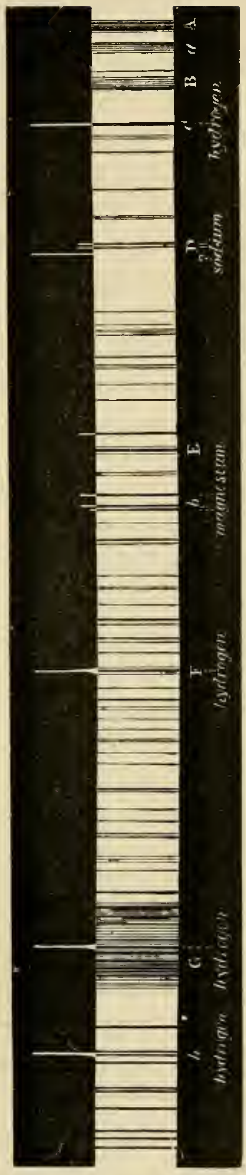

FIG. 44.—Spectrum of the sun's photosphere (below) and chromosphere (above), showing the lines ordinarily visible, and that they extend to different heights.

layer, first of all, a layer of hydrogen with its prominences, and then at the bottom of this layer another layer of magnesium, which wells up sometimes where the prominences are most active.

The different lines we see in our instruments when we observe the solar prominences are not all alike. Some of the lines vary very much from the appearance of the C line of hydrogen, for instance. In fact, in one line, the F line (see Fig. 43) we get a trumpet-shaped appearance. The line widens as it approaches the sun, so that it resembles an arrow head, resting on the thin absorption line which forms the shaft. This is not only true of the hydrogen lines, but of the lines due to the injection of other substances into the sun's outer atmosphere from below.

This artificial eclipse method does more than reveal the heights and chemical nature of the prominences. A slight extension of it enables us to see their actual forms and watch the tremendous changes going on in them from time to time. The first observation of this nature was made in 1868[1] by causing the narrow slit, radial to the edge of the sun, to pass slowly over the prominence. By this means a number of sections of varying length was obtained, which, placed side by side, gave an idea of its shape. It is sufficiently obvious that in this way a perfect view of the prominence can only be obtained by moving the slit with sufficient rapidity to allow of persistent images. At the very outset Janssen and I attempted to accomplish this, Janssen by giving a rotatory motion to a direct-vision spectroscope, I by giving an oscillating motion to the slit, in which plan I was followed by Young,[2] who afterwards expanded it. Although these plans were successful, they were subject to the disadvantages of reducing the quantity of light and shaking the instrument.

The method now adopted for viewing the forms of the prominences is one of extreme simplicity, the principle of which was first published by Zöllner,[1] in February, 1869, and adopted by myself before I had heard of his paper. The method consists simply in the use of a widely opened slit. Zöllner, however, fearing that the quantity of light admitted would be so great as to obscure the forms of the prominences, proposed to reduce it if necessary by polarising or absorbing media placed in the eyepiece.

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[1] *Proc. Roy. Soc.* No. 105, 1868.

[2] *Nature*, Dec. 8, 1870.

On the 16th of the same month in which Zöllner announced his idea, Mr. Huggins [2] suggested a similar means of attaining the same end by a combination of an open slit and ruby glass.

On the 29th, I heard of this paper of Mr. Huggins', and it at once struck me that the absorptive media were useless, and that the same result might be accomplished by the use of the open slit only if the field was kept sufficiently small. On putting this to the test I found my expectation fully confirmed.[3]

The way this method is applied will be readily understood.

Having found a prominence and got it carefully focused for the C or F line, the slit is then simply opened widely, when it is found that all the details of the prominence can be seen. The reason of this will be clear on a little consideration. We have already seen that the hydrogen Fraunhofer lines (like all the others) appear dark because the light which would otherwise paint an image of the slit in the place they occupy is absorbed; but when we have a prominence on the slit, there is light to paint the slit; and as in the case of any one of the hydrogen lines we are working with light of one refrangibility only, on which the prisms have no dispersive power, we may consider the prisms abolished. Further, as we have the prominence image coincident with the slit, we shall see it as we see the slit, and the wider we open the slit the more of the prominence we shall see. We may use either the red, or green, or blue light of hydrogen for the purpose of thus seeing the shape and details of the prominences; how far the slit may be opened depends upon the purity of the sky at the time.

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[1] *Astronomische Nachrichten*, No. 1772, September 15th, 1869.

[2] *Proc. Roy. Soc.* vol. xvii. p. 302.

[3] *Ib.* No. 110, 1869.

In 1873, Mr. Seabroke and myself communicated a method to the Royal Society,[1] which enabled us to observe the whole of the chromosphere at once by means of a ring slit, which was made coincident with an image of the chromosphere itself, so that the light from the sun itself was stopped out. This arrangement will be readily understood on referring to the diagrams (Figs. 45 and 46).

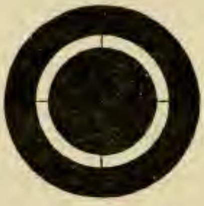

FIG. 45.—Diaphragm showing annulus, the breadth of which may be varied to suit the state of the air.

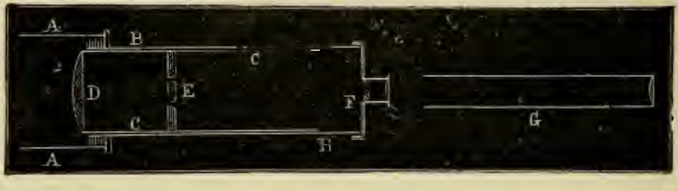

FIG. 46.—The annulus is viewed and brought to focus by looking through apertures in the side of the tubes. A, sliding eye-tube of telescope; B, tube screwing into eye-tube; C, tube sliding inside B, and carrying lens D and diaphragm E; F, lenses bringing image of diaphragm to a focus at the place generally occupied by the slit of the spectroscope; G, collimator of spectroscope.

It afterwards came to our knowledge that Zöllner had conceived the same idea, unknown to us, but had rejected it; and that Professor Winlock had tried a similar arrangement.[2]

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[1] *Proc. Roy. Soc.* vol. xxi. p. 105.

[2] For a more complete account of the methods of viewing the forms of the prominences, see *Solar Physics*, p. 578 *et seq.*

The different forms which these prominences assume sometimes are very striking. Here one is reminded, by the fleecy, infinitely delicate cloud-films, of an English hedge-row with luxuriant elms; here of a densely intertwined tropical forest, the intimately woven branches threading in all directions, the prominences generally expanding as they mount upwards, and changing slowly, indeed almost imperceptibly.

It does not at all follow that the largest prominences are those in which the intensest actions, or the most rapid change is going on. The greatest action is generally confined to the regions just in, or above, the chromosphere.

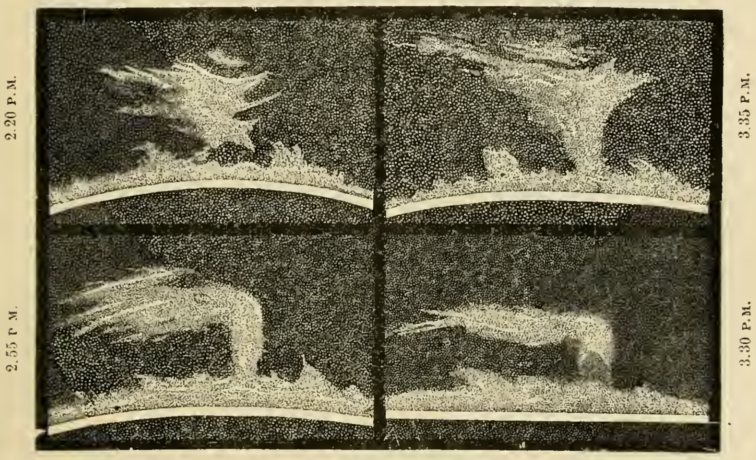
2.20 P.M.
3.35 P.M.
2.55 P.M.
3.30 P.M.

FIG. 47.—Solar prominences (Young), showing lateral currents.

This and subsequent observations led me to propose a division of prominences into two classes:—[1]

1. *Eruptive.*—Those in which great action is going on, lower vapours being injected; in the majority of cases these are not high, they last only a short time—are throbs, and are oft renewed. They often accompany spots, but are not limited to

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[1] *Proc. Roy. Soc.* No. 120, 1870.

them, and are not seen so frequently near the sun's poles as near the equator.

2. *Nebulous.*—Those which are perfectly tranquil, so far as wave-length evidence goes. They are often high, are persistent, and not very bright. These do not as a rule accompany spots.

This classification was afterwards accepted by Secchi, Zöllner, Spörer, and Young, and also by Respighi, who, however, divides the eruptive class into a great many sub-classes.

Now, not only are these differences in the forms of the prominences associated with differences in their chemical constitution, but by observing the form of each prominence, first by means of a line due to one substance and afterwards by means of lines due to others, we can get actual sections of the prominence showing the parts occupied by the substances producing the different lines. The importance of such observations we shall find it difficult to overrate.

Detailed observations of the chromosphere showed that its free edge was not only subject to those great local disturbances which form the prominences, but that along its whole length it was subject to the same action in a smaller degree. The edge of the chromosphere is never smooth, but irregular, and the forms of the irregularities are found to conform to one or other of the two types to which we have seen the prominences may be referred. Sometimes it is marked by gentle undulations, at other times thrown into sharp jet-like projections.

Since the introduction of the new method, several workers, pre-eminent amongst whom Professors Respighi, Secchi and Tacchini must be mentioned, have carried on the observation either of the forms and details or the chemical composition of the prominences with indefatigable perseverance, so that as the result of their labours we have an almost daily record of all the prominences visible.

It was at first thought impossible that prominences could thus be seen on the disc of the sun itself,[1] as this was not in accordance with Kirchhoff's hypothesis. Subsequent observations, however, soon showed that this is quite possible.[2]

The first observations of this nature were made by Father Secchi and myself almost simultaneously. My observation was made on the 11th of April, 1869, and Secchi's letter to the Paris Academy announcing the same appearance is dated the 13th.

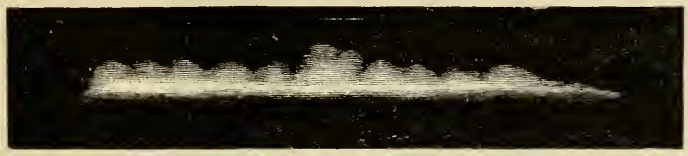

FIG. 48.—Edge of chromosphere (billowy).

This reversal of the hydrogen lines on the sun's disc was also observed by Captain Herschel on June 10th, 1869, and by Professor Young, who in a communication to the Franklin Institute, dated October 3rd, 1870,[3] thus describes the reversal of the C and F lines in a group of spots then visible:—

> "At 4.05 P.M. the brilliance of the F line increased so greatly that it occurred to me to widen the slit, and to my great delight I saw upon the disc of the sun itself a brilliant cloud, in all its structure and detail identical with the protuberances around the limb. Indeed, there were two of them, and there was no difficulty in tracing out and delineating their form. Fig. 50 represents them

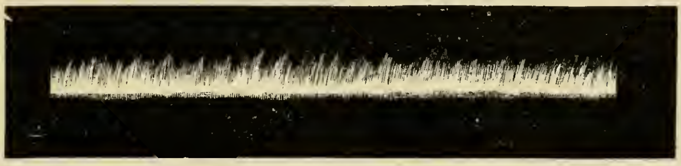

FIG. 49.—Edge of chromosphere (pointed).

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[1] *Proc. Roy. Soc.* No. 110, 1869. See also Secchi, *Comptes Rendus*, vol. lxviii. p. 237.

[2] *Proc. Roy. Soc.* vol. xvii. p. 415.

[3] *Nature*, vol. iii. p. 113.

as they were from 4.05 to 4.10; Fig. 51 gives the form at 4.15—4.20. They were then considerably fainter than at first. During the intervening ten minutes I examined the other lines of the spectrum, and found that the form could be distinctly made out in all the *hydrogen* lines, even in *h*; but that the reversal of the other lines, including D_3, was confined to the region immediately over the spot-nucleus, where the smaller but brighter cloud terminated abruptly, or, I might better say, *originated*. The larger one faded out at both ends. When the clockwork of the equatorial was stopped, the luminous cloud took 16·7 seconds of time to traverse the slit which was placed parallel to the hour-circle. This indicates a length of at least 130,000 miles, without allowing anything for the foreshortening resulting from the nearness to the sun's limb.

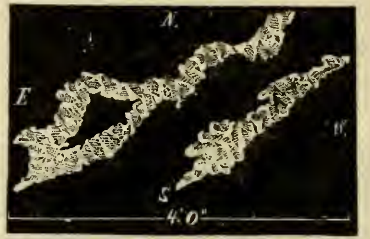

FIG. 50.—The prominence at 4.05.

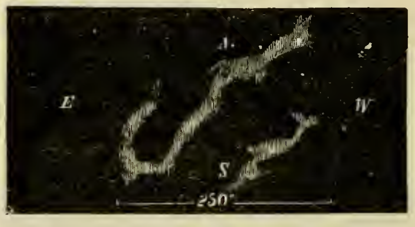

FIG. 51.—The prominence at 4.20.

2. *Contortions of Spectral Lines.*

In my first observations of the chromospheric lines by the new method,[1] I was greatly puzzled to account for the occasional strange behaviour of the F line. Its coincidence with the corresponding dark line could not always be satisfactorily made out—it appeared to be more refrangible. Again, a few days later, when observed with a tangential slit, the line assumed the appearance shown in Fig. 52.

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[1] *Phil. Trans.* 1869, p. 425.

In the spectrum of the light proceeding from the exact limb of the sun the bright line was seen more refrangible than F, but in the spectrum of the prominence at some distance above the sun the black line F was eclipsed. Hence it appeared that away from the sun's surface the substance which produced the line gave out light of less refrangibility than it did at the surface.

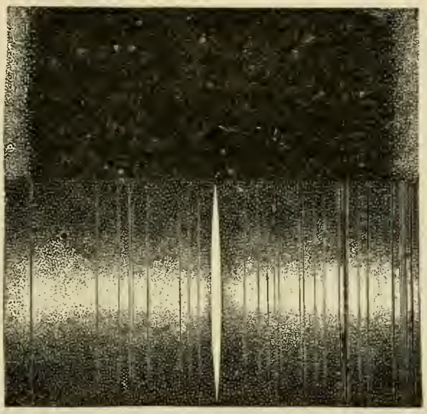

FIG. 52.—Non-coincidence of bright and dark F line when tangential slit is used, and when both photospheric and chromospheric light is admitted.

Again, the bright line was sometimes strangely contorted, as shown in Fig. 53, sometimes being violently deflected to the right or left and sometimes broken as in Fig. 54, part of it being torn away considerably to the more refrangible part of the spectrum. We have in the first figure the F line. The slit—the perfectly straight slit—has been worked round the limb in search of a prominence, and it has found one. But the slit is no longer shown us as a perfectly straight line, it is in fact a very irregular one; and further than this, it branches at a certain distance from the sun and becomes double.

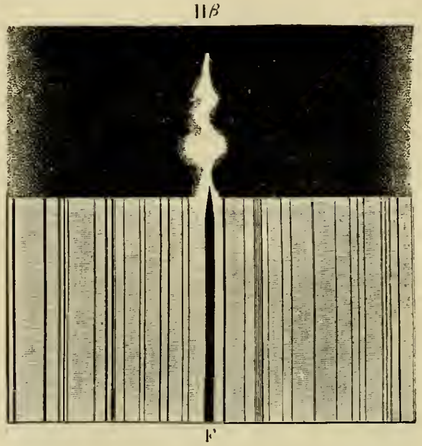

FIG. 53.—Contortions of F line in a prominence.

The next series of figures (Fig. 55) represents another observation of this bright F line of hydrogen. These various effects

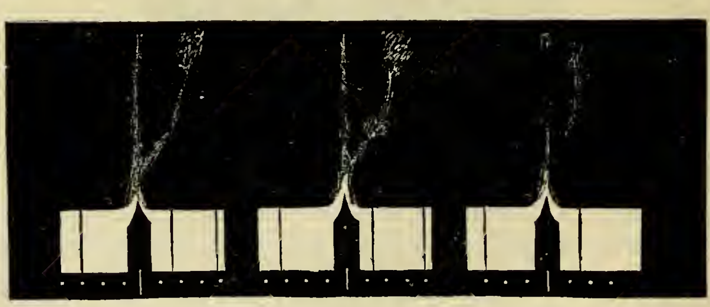

FIG. 54.—Contortions of the hydrogen lines.

were produced by varying the position of the slit a very little indeed over a small and very brilliant prominence.

The left-hand figure shows the appearance of this line when the slit was just on one edge of the prominence, the central figure gives its appearance when it was entirely included in the slit, and the right-hand figure when the slit was on the opposite edge.

There was another important fact connected with this: when the phenomena were observed close to the limb it was very often seen that the dark line on the surface of the sun was broken; in fact we got a doubling of the dark F line in exactly the same way as we got this doubling of the bright line in the prominence itself.

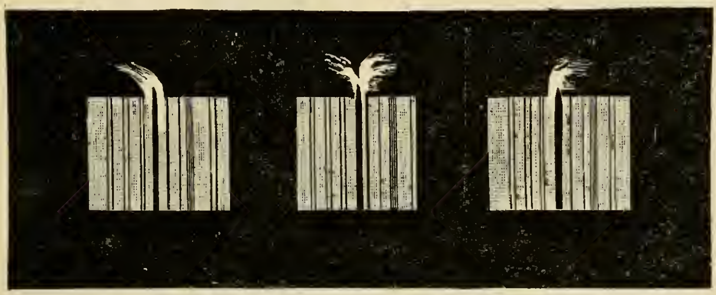

FIG. 55.—Contortions of the hydrogen lines. The right-hand side of each diagram is the most refrangible.

The next point observed was (and this was an observation very difficult indeed to make near the limb) that whenever we got any very considerable activity we got a new order of phenomena altogether, indicated in the two diagrams (Figs. 56 and 57).

It was found that the absorption of the hydrogen, or of the magnesium, or of the sodium, as the case might be, was enormously reduced; that for that part of the sun there was practically no absorption; but instead of absorption an excessive brilliance in that part of the spectrum where the dark line would otherwise have been. In the brighter portion between the two small spots (Fig. 57) the absorption was replaced by an exceedingly brilliant radiation, so brilliant, indeed, that it is quite impossible to draw a diagram so as to give any idea of the intense brightness of some of these little spots of light which one sees in the spectroscope; they fatigue the eye enormously, although they cover such a very small portion of the field of view, and with these the straight and evenly-bounded image of the slit had given way to an irregular one.

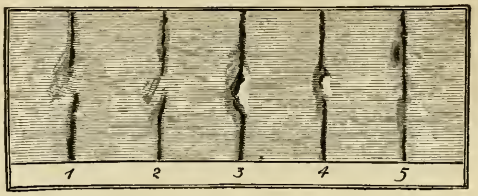

FIG. 56.—Contortions of F line on disc. 1 and 2, rapid downrush and increasing temperature; 3 and 4, uprush of bright hydrogen and downrush of cool hydrogen; 5, local downrushes associated with hydrogen at rest.

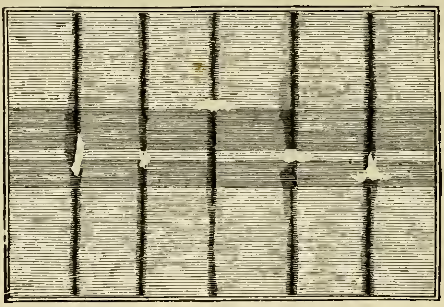

FIG. 57.—Contortions of F line on disc, in connection with spots and uprushes of bright hydrogen.

Accompanying this intense radiation there was a gradual fading away of the absorption line; it waned, and faded, and became almost invisible; while, on the other hand, on the other side or in other places, instead of getting a brilliant patch of light of the same width as the F line, we got one many times broader. We had also the absorption deflected to the left, or red end of the spectrum, and on that side it was

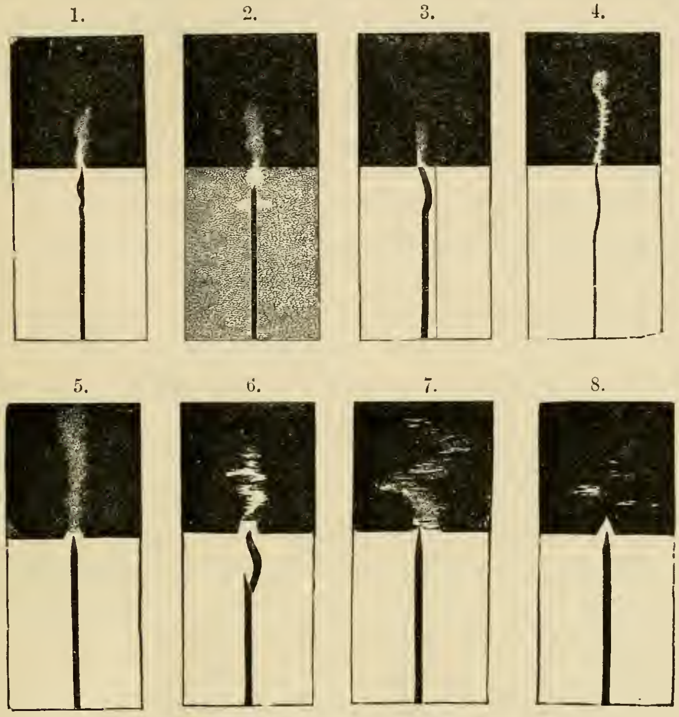

FIG. 58.—Motion forms and lozenges.

Mr. Holiday's drawings and remarks.
1. Prominence much bent.
2. Prominence encroaching over limb—bright line crossing black line.
3. Black line (F) curved downwards, sometimes nearly touching iron line below.
4.
5. Prominence nearly divided.
6, 7, 8. My own drawings.

gradually fined or eased off, so that it was very difficult to determine exactly where this broadened, deflected F line actually ceased to give us absorption; whereas at the other side where it changed its refrangibility towards the blue end of the spectrum, we had an enormous patch of light.

An observation which I made in April, 1870,[1] pointed unmistakably to a connection between these bright regions or lozenges, as I called them, and the prominences. On examining the F line, I found that the bright chromospheric line not only intruded very much more than it generally did on to the disc, but that close to it I saw one of those bright lozenges to which reference has just been made, the dark hydrogen absorption appearing over it. Gradually this lozenge travelled very nearly to the edge of the sun, and before it got quite to the edge, I saw a tremendous contortion of the F line, that line being deflected first violently to the violet, and then as violently to the red end. Also, in the same locality, I saw the F line broken into two parts—it was doubled. And what was going on while this was happening? A prominence, obviously with its root some distance from the limb, had gradually travelled beyond the limb; in appearance it became very much more elevated, and seemed, as it were, in perspective over the limb; but what I saw first was very rapidly changed. I was not observing with an open slit, so I at once coined the term, "motion forms," because the forms observed did not in any way represent the shape of the prominences.

The publication of these observations gave rise to much discussion. Professor Respighi attributed the phenomena to disturbances in our air, or to the action of the heat of the sun's image on the slit, apparently forgetting that such a cause would affect all the lines in the spectrum, and not a few isolated ones only. In a subsequent memoir he admits having himself seen the phenomena. Father Secchi also for a long time discredited it, but eventually admitted that he had seen the phenomena. Another observer referred to my observations on the changes in question as "les illusions de Mr. Lockyer"; but Professor

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[1] *Proc. Roy. Soc.* No. 120, 1870.

Young from the first observed and recorded exactly similar phenomena.

At length, after all this discussion, the new phenomena have been decided to be truly solar in their origin, and not due to any instrumental or other errors, as had been suggested.

CHAPTER X.

PRELIMINARY SEARCH AFTER EXPLANATIONS.

1. *The Widening of Lines.*

In the preceding chapter I have attempted to give a rapid and general sketch of the world of wonders in which we had been landed by the new method of solar observation. It became obviously of first importance to attempt to get some explanation of those phenomena, at all events, which were considered abnormal because they were new. It was also obvious that if laboratory experiments could not throw light upon such appearances, for instance, as the widening of the lines in spots and at the base of some of the chromospheric lines, we should be in an *impasse.*

With a view to get new facts, and among them some bearing upon the widening of spectral lines, Dr. Frankland and I commenced, in 1869, a series of researches on gaseous spectra.[1] As the result of this inquiry, which extended over a period of three years, we came to the conclusion that laboratory experiments could help us—in fact, Dr. Frankland had already succeeded in giving great width to the hydrogen lines,[2]—and that it was possible experimentally to vary the thickness of lines in the way we find them thickened in the sun. The means by which this was effected was an increased quantity or pressure of the vapour producing them. We found, for instance, that by increasing the pressure, say of hydrogen, and rendering the gas incandescent by means of the passage of an electric current, we thickened the lines, and especially the F line, exactly as it is thickened in the lower region of the chromosphere, and as some lines are thickened in spots. We carried on the experiments to a pressure of twenty atmospheres, and the spectrum was continuous with maxima indicating the places of the lines.

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[1] *Proc. Roy. Soc.* vol. xvii. p. 288.

[2] These observations were never published, but in a paper communicated to the Royal Society in 1868, Dr. Frankland described a series of experiments on the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. In the case of hydrogen he found that under a pressure of two atmospheres "the previously feeble luminosity is

Since by varying the pressure of hydrogen we can thus vary the thickness of the lines, it is possible to observe the spectrum of hydrogen in a tube, and to place the spectrum we get from the hydrogen in the sun, side by side with the spectrum we obtain from the hydrogen in the tube. We can vary the pressure of the hydrogen in the tube so that its spectrum exactly fits, so to speak, the spectrum of the hydrogen in the sun; and hence we are enabled to determine the pressure of the atmosphere at the sun. And this, no doubt, will some day be done, but the thing is not quite so easy as this. There are more chemical substances than hydrogen in that part of the sun which so conveniently gives us these bright lines; and we have in all questions of pressure not only to take into account the actual pressure of the hydrogen, but the combined pressure, so to speak, of all the vapours which exist in that stratum; and so we have a very great inquiry before us, before a final estimate of the pressure can be given.

very visibly augmented, whilst at ten atmospheres' pressure the light emitted by a jet about one inch long is amply sufficient to enable the observer to read a newspaper at a distance of two feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*"—*Proc. Roy. Soc.* vol. xvi. p. 419.

It happened, however, that in the Geissler tubes employed when the molecules of the gas were most agitated so as to give the phenomena of great pressure, the current might be held to give us the highest temperature. It was therefore of importance to eliminate electrical effects altogether. This was accomplished by volatilising a piece of sodium in a glass tube in an atmosphere of hydrogen and observing its *absorption* spectrum. The experiments were conducted as follows:—[1]

(1.) Into a piece of hard glass combustion-tube, thoroughly cleaned and closed at one end, a few pieces of metallic sodium, clean and as free as possible from naphtha, were introduced. The end of the tube was then drawn out and connected with a Sprengel pump and exhausted as rapidly as possible. Hydrogen was then admitted, and the tube re-exhausted, and when the pressure was again reduced to a few millimetres, carefully sealed up. The tube thus prepared was placed between the slit plate of a spectroscope and a source of light giving a continuous spectrum.

Generally, unless the atmosphere of the laboratory was very still and free from dust, the two bright D lines could be distinctly seen on the background of the bright continuous spectrum.

The tube containing the sodium was then heated with a Bunsen flame and the spectrum carefully watched. Soon after the application of the heat, a dark line, thin and delicate as a spider's thread, was observed to be slowly creeping down each of the bright sodium lines and exactly occupying the centre of each. Next, this thin black line was observed to thicken at the *top*, where the spectrum of the *lower* denser vapours was observed, and to advance downwards along the D line, until, arriving at the bottom, they both became black throughout; and if now the heat was still applied, thus increasing the density of the various layers of the sodium vapour, the lines began to broaden until, in spite of considerable dispersion, the two lines blended into one. The source of heat being now removed, the same changes occurred in inverse order; the broad band split into two lines, gradually the black thread alone was left, and finally that vanished, and the two bright lines were restored.

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[1] *Phil. Trans.* 1872, p. 253.

(2.) This experiment was then varied in the following way. Some pieces of metallic sodium were introduced into a test-tube, and a long glass tube conveying coal-gas passed to the bottom, an exit for the gas being also provided at the top. The sodium was now heated and the flow of coal-gas stopped. In a short time the reversal of the D lines was complete. The gas was now admitted, and a small quantity only had passed when the black lines were reduced to threads.

In this manner we were able to artificially reproduce the thickness observed in the D lines when they are thinnest in a high prominence and thickest in a deep spot, and this in a way which eliminated the effect of temperature.

When we consider the widening of the lines seen in spots we are in presence of another cause than pressure to which that widening may be ascribed. There are many reasons for believing that a spot is the seat of a downrush of comparatively cool vapours, so that in it we have an accumulation of those vapours, and the increased quantity of any particular substance leads to a widening of its spectral lines independently of the change in the same direction that would be produced by the pressure which also is doubtless increased.

Experimentally we can vary the thickness of the D line of sodium so as to exhibit the effects to a large audience, and if we then examine the conditions under which this is effected, we get an additional strengthening of the above explanation of the thickening of the lines.[1]

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[1] It is the more necessary to insist upon this, as quite recently the thickening has been attributed by M. Fievez to temperature. This would make the temperature of the sun-spots lower than that of the sodium vapour in the experiment referred to above!

The experiment is managed as follows. The condenser of a Duboscq lantern being removed, a combination of a slit and lens is put in its place, the slit being placed inside the lantern close to the carbons at the principal focus of the lens. The carbons, about $\frac{3}{4}$ of an inch in diameter, have their ends filed perfectly flat, and a very thin layer of metallic sodium is spread on the lower pole.

The rays of light proceeding through the slit fall upon the lens, which renders them parallel, and then enter two hollow prisms filled with carbon disulphide. They are next focused by means of a lens of long focus upon a screen.

When the two poles are brought together the sodium line is seen to be very strongly reversed upon the screen, the line being about 15 inches wide in a spectrum of between 3 and 4 yards long, and it remains so, as long as the carbons are nearly in contact, till all the sodium is volatilised. By separating the poles the absorption line can be thinned down until it becomes a fine line, and then, by opening them a little more, the line can be obtained as a bright one.

The cause of this is perfectly obvious. The temperature is practically the same all the time, but we have a very considerable quantity of sodium vapour surrounding the incandescent poles in the first instance. On the continued application of the heat this sodium vapour goes away by degrees, and we gradually deal with a smaller quantity, and as we deal with a smaller quantity the line thins. We therefore are justified in saying that when in a sun-spot we get the line of sodium considerably thickened, that is due to the fact that there is a greater quantity of sodium vapour present in that spot.

These principles afford a satisfactory explanation not only of the widening of F and other lines near the limb, but of those bright patches or lozenges which are sometimes seen interrupting or accompanying the dark hydrogen lines on the surface of the sun itself, especially in the locality of spots. The fact that the lines are bright indicates that the hydrogen which produces them is more intensely heated than usual, and we may assume, therefore, that it comes from below—the region of higher temperature. We can therefore regard them as enormous ejections, or uprushes of hydrogen, so intensely hot that it radiates much more light than it absorbs. This gradually replaces the absorbing hydrogen, which is driven down again with such a considerable velocity that it commonly suffers a displacement towards the red, while the hot ascending hydrogen undergoes a similar alteration in the direction of the violet.

In the great width of some of these bright patches we find a parallel phenomenon to the widening of the bases of the chromospheric lines and of the lines in spots, and this resemblance naturally leads to the suspicion that all these phenomena may be due to some common cause, that cause, as we have seen in the case of spots and chromospheric lines, being increased pressure or quantity of any particular vapour.

2. *The Simplification of Spectra.*

We have already seen that the spectrum of the chromosphere is far simpler than the general solar spectrum, and that the lower regions of the chromosphere are more complex than the upper reaches. It was next observed that the spectrum of the same substance varied at different distances from the sun. Of the hydrogen lines, F was found to be the longest,[1] so that at a certain elevation all the other lines were left behind, and we had a stratum in which hydrogen was represented by a single line only. Further observation disclosed the same fact in the case of magnesium. Its lines b^1, b^2, were of nearly equal height, while b^4 was much shorter.

To explain these facts Dr. Frankland and I continued our researches on gaseous spectra, and found that under certain conditions of temperature and pressure, the very complicated spectrum of hydrogen could be reduced in the laboratory *to one line in the blue-green* corresponding to F in the solar spectrum; and also that the equally complicated spectrum of nitrogen is similarly reducible to one bright line in the green, with traces of other more refrangible faint lines. From a mixture of the two gases we obtained a combination of the spectra in question, the relative brilliancy of the two bright green lines varying with the amount of each gas present in the mixture; and by removing the experimental tube a little further away from the slit of the spectroscope, the combined spectra were reduced to the two bright lines. By reducing the temperature all spectroscopic evidence of the nitrogen vanished; and by increasing it, many new nitrogen lines made their appearance, the hydrogen line always remaining visible.[1]

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[1] See letter to Mr. Warren De La Rue, Oct. 23rd, 1868, printed in *Proc. Roy. Soc.* No. 105, 1868.

These experiments enabled us at once to connect the two series of observations.

It was only necessary, in fact, to assume that, as in the case of hydrogen and nitrogen, the spectrum became simpler when the density and temperature were less, to account at once for the reduction in the number of the lines visible in those regions where the pressure and temperature of the absorbing vapours of the sun would be reduced.

The results of the continuation of this line of inquiry will be stated further on.

3. *The Contortion of Lines.*

Those strange contortions which as we have seen are commonly observed in the bright chromospheric lines, and in the dark lines on the disc of the sun, were at first a very great puzzle, not only to me, but to two eminent physicists whom I consulted at the time. The accompanying phenomena, however, soon suggested the explanation which, unknown to me then, had been applied by Secchi and Huggins, a few months earlier in the same year, to somewhat similar effects observed in the spectra of stars. The explanation depends upon a view first advanced by Doppler in 1842, that the light from a moving light-source is not the same in all its qualities as light from a fixed one.

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[1] *Proc. Roy. Soc.* No. 112, 1869.

Doppler's idea will be gathered from the following analogies from sound:—

The colours which we see in the spectrum are exactly analogous to the notes which we hear in a piano when we go from one end of the scale to the other. Doppler imagined the equivalent of a piano going away from or coming towards the listener with considerable velocity—a velocity comparable, in fact, to the velocity of sound in air. It is clear that under these circumstances we should no longer get true concert pitch *for each note*, since the note which gives us a certain tone, because it produces in the air so many waves per second, will change its tone if the source of the note is coming to us. Take, for instance, a tuning-fork giving concert C, and imagine it rapidly coming to us: the waves of sound will be crushed together, we shall have more waves in a second falling on the ear, and we shall get a higher note. If we imagine, on the other hand, the tuning-fork to be going away from us, the notes will be paid out at longer intervals, so to speak, and we shall get a lower note. In neither case shall we continue to have concert C.

A very familiar instance in which we do get this change of pitch due to change of motion, is produced in these days of very rapid railway travelling. Any of us who have been at a country railway station when an express is coming by will know that as the train approaches us the note of its whistle is at one pitch, and as it goes from us after passing, it changes and gives a lower note according to the velocity of the train. An experimental illustration of this principle is to attach a whistle to the end of a long india-rubber tube. If then a person sounds the whistle by blowing through the open end of the tube, and while still blowing whirls it round rapidly in a vertical plane in which an observer is standing, that observer will note that when the whistle is approaching him in one part of the curve, and the waves are therefore being crushed together, the note will appear higher than when it is receding from him in the opposite part of the curve, where the waves are being, as it were, pulled asunder.

Now to apply this to the light from the hydrogen prominences in which the effects are most pronounced. The long notes of light are red, and the short notes are blue, and if we sharpen or shorten any light note in any part of the spectrum we shall give that light a tendency to go towards the blue, and if we lengthen or flatten it we shall give it a tendency to go towards the red; so that, for instance, if a mass of hydrogen gas giving the line or note in the green indicated by F, is approaching us with a velocity comparable to the velocity of light, the line will change its position in the spectrum towards the blue; and if we are careful to note the exact amount of change of refrangibility, as it is called, we shall have then an absolute method of determining the rate of relative motion of that mass of gas. This will help us in more ways than one. Suppose we observe the gas at the limb of the sun, we shall then, if we get any change of refrangibility, be justified in calling it a solar wind, because the motion thus indicated would be very nearly parallel to the surface of the sun; but if on the disk of the sun itself—take a spot, for instance, in the very middle of the disk —we get any change of wave-length such as I have referred to, it is perfectly clear that we shall no longer be dealing with what we can justly call a wind, it will really be an upward or downward current. So that this principle enables us at the limb of the sun to determine the velocity of solar winds, and at the centre of the sun to determine the velocity of uprushes or downrushes.

If the hydrogen lines were invariably observed to broaden out on both sides, the idea of movement would require to be received with great caution; we might be in presence of phenomena due to greater pressure, both when the lines observed are bright or black upon the sun; but when they widen out sometimes on one side, sometimes on the other, and sometimes on both, this explanation was soon seen to be untenable, as Dr. Frankland and myself in our researches at the College of Chemistry had never failed to observe a widening out on both sides of the F line when the pressure of the gas had been increased.

In the explanation, I have been compelled to refer to a light-note, and as a matter of fact, on the sun the phenomena are limited to certain lines. It was on this point that Doppler was in error; he dealt with the total light, which could never be changed in consequence of the change of motion he assumed. The true criterion was first pointed out by Fizeau, who showed that the change must take place at a particular wave-length to be noticeable at all.

M. Fizeau's researches in this direction are so little known in this country that I give the account of them at length in a foot-note.[1]

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[1] "Si un corps sonore émettant un son continu et toujours identique se meut avec une vitesse comparable à celle du son, les ondes sonores ne seront pas symétriquement disposées autour du corps sonore, comme cela à lieu lorsqu'il est au repos; mais elles seront plus rapprochées les unes des autres dans la région vers laquelle aura lieu le mouvement, et plus éloignées dans la région opposée, pour un observateur placé en avant ou en arrière du corps sonore; le son sera donc différent, plus aigu dans la première position, plus grave dans la seconde.

"Si l'observateur à son tour est supposé en mouvement, le corps sonore restant immobile, le résultat sera semblable, mais la loi du phenomène est différente.

"En calculant les vitesses qui correspondant aux intervalles de la gamme on trouve les nombres suivants: pour produire une élévation d'un *demi-ton*, le corps sonore doit avoir une vitesse par seconde de 21·25, pour un ton majeur 37·8, pour la tierce 68, pour l'octave 170. Dans le cas du corps sonore immobile et pour obtenir les mêmes notes l'observateur doit avoir les vitesses 22·6, 42·5, 85, et 340. Les sons émis ou recus dans des directions différentes de celles du mouvement se calculent en projetant la vitesse sur la nouvelle direction.

"L'auteur donne la description d'un appareil qu'il a employé et au moyen duquel on peut vérifier et démontrer commodément ces curieuses propriétés du son,

To see the bearing of this, let us suppose that one of the constituents of the solar atmosphere, let us say hydrogen, while it is giving out its light is moving towards us at the rate of say fifty miles (= 80,466 metres) a second. Now the wave-length of one of the lines due to hydrogen, F, is, as we have seen, 0·00048606 at the normal velocity. With a higher velocity, therefore, the number of crests per second reaching the eye must be greater, and therefore *the effective wave-length must be shorter.* In the spectrum this shortening of the wave is indicated by the position of the line F changing towards the violet —the region of shorter waves.

If we supposed the hydrogen receding from the eye, then the

dans le cas du mouvement du corps sonore. Cette appareil est fondé sur le principe des roues dentées de M. Savart, mais la disposition est inverse. Au lieu de dents mobiles recontrant dans leur mouvement un corps élastique fixe, c'est le corps élastique qui est placé sur la circonférence d'une roue et qui recontre dans son mouvement des dents fixes placées sur la concavité d'un arc extérieur immobile, l'on a ainsi un appareil fixe qui jouit de la propriété d'émettre des sons différents dans chaque direction particulière. Pour une certaine vitesse de rotation par example on aura en avant le son fondamental, en arrière l'octave, et toutes les notes de la gamme dans des directions intermédiares.

"En appliquant ces considérations à la lumière on arrive à des consequences curieuses et qui pourraient acquerir de l'importance si l'experience venait à les confirmer. Un mouvement très-rapide et comparable à la vitesse de la lumière, attribue au corps lumineux ou à l'observateur, aura pour effet d'altérer la longueur d'ondulation de tous les rayons simples qui composent la lumière reçue dans la direction du mouvement. Cette longueur sera augmentée ou diminuée suivant le sens du mouvement. Considéré dans le spectre, cet effet se traduira par un *déplacement de raies* correspondant au changement de la longueur d'ondulation.

"En calculant la value du déplacement angulaire de la raie D dans la cas ou le corps lumineux aurait la vitesse de la planète Vénus, le spectre étant formé au moyen d'un prisme de flint de 60° on trouve 2″·65.

"Pour le cas où l'observateur seul serait en mouvement et animé d'une vitesse égale à celle de la terre on trouve 2″·25.

"En supposant que l'on mesure les déviations doubles et que l'on se place successivement dans des conditions où les mouvements en question seraient de signe contraire, ces quantites peuvent être quadruplées, et l'on a 10″·6 et 9 pour les valeurs précédentes.

"L'auteur termine en examinant si ces conséquences pourront être soumises à l'observation, et il pense que les difficultés ne sont pas telles qu'on ne puisse espérer de les surmounter."—Société Philomathique de Paris. *Extraits des Procés-verbaux des Sciences,* 23 Dec. 1848, p. 81.

position of the line would be changed towards the region of longer waves—*i.e.* towards the red.

Let us suppose such a change to be observed, say a change of the F line, the normal wave-length of which is 0·00048606 from that position to 0·00048716. Obviously the wave has been lengthened by the *recession* of the source of light from the eye, and the amount of recession, about thirty-nine miles a second, is measured by the increased length of wave, the difference in the wave-length bearing the same ratio to the total wave-length as the difference in the velocity bears to the velocity of light.[1]

We can now interpret the meaning of the strange doubling of the F line shown in Fig. 54. We there get, according to the principle just laid down, an indication of the fact that the hydrogen up to a certain height was very nearly at rest, and that beyond, part of it was torn away, the line being deflected towards the blue, indicating that it was approaching us. Now the Fraunhofer lines in the diagram may be looked upon as so many milestones which enable us to measure by the deflection the number of miles traversed by the gas in one second; for these deflections are nothing more nor less than alterations of wave-length, and, thanks to Ångström's map, we can measure distances along the spectrum in $\frac{1}{100000000}$ mm., and we know that an alteration of $\frac{1}{100000000}$ mm. in the wave-length of the F line towards the violet means a velocity of thirty-eight miles a second towards the eye; and that a similar alteration towards the red means a similar velocity from the eye; so that carrying the part of the line which has the greatest deflection from the normal down to the dots, we find that the velocity of the solar wind under observation at that time was something like 114 miles per second.

In the second figure this same prominence is seen a short time afterwards. The tremendous rush of hydrogen has descended somewhat nearer the sun, and bringing that in the same way down to our milestones, we can give that velocity at something like fifty miles per second. The wind velocities measured in this way have amounted to 140 miles a second, while the convection currents give us velocities which very often amount to forty or sixty miles a second.

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[1] See Clerk-Maxwell, *Phil. Trans.* 1868, p. 532.

This method enables us to determine a matter which a few years ago we could not have determined in any other way. I refer to the fact that the motions of the solar winds are to a very great extent cyclonic.

Referring again to the series of drawings (Fig. 55), it will be seen that in the first of these drawings the hydrogen line indicates by its change of refrangibility towards the red that the gas is receding from us. In the third diagram we see that in that part of the prominence the rays were being deflected towards the violet; that is to say, they were approaching us. In the middle drawing, which represents what was seen when the entire cyclone was included in the slit, we get indications both of recess and approach.

Now if anybody in the moon had as good a method as this of measuring an earthly cyclone, he would see exactly this sort of thing—the part of the cyclone receding from him would give a deflection in one direction, *a complete view of the cyclone would give him both deflections,* because he would get currents going in both directions, and on the other part of the cyclone he would get a deflection in the other direction.

CHAPTER XI.

THE NEW METHOD APPLIED TO LABORATORY WORK.

Attention must next be drawn to another method of observation of spectra, or rather to the extension of the method we have seen applied to the localization of solar phenomena, to a different line of work.

Kirchhoff, as we have seen, examined the sun as a whole, and compared it with the light of a light-source as a whole. We have stated the difference in the results obtained when we pass from the method of observing the sun as a whole to that other more detailed one of observing a small portion at a time.

Now is it worth while to do this with the light-source ?—that is the question. Let us deal with some simple considerations which should enable us to give an answer to this question.

The spectroscope, however simple or complex it may be, is an instrument which allows us to observe the image of the slit through which the light enters it, in the most perfect manner. If the light contains rays of every wave-length, then the images formed by each will be so close together that the spectrum will be continuous, that is, without break. If the light contains only certain wave-lengths, then we get certain, and not all, of the possible images of the slit, and the spectrum will be discontinuous.

Again, if we have an extremely complex light-source, let us say a solid and a mixture of gases giving out light, and we allow the light to enter, so to speak, indiscriminately into the spectroscope, then in each part of the spectrum we shall get a summation—a complex record—of the light of the same wave-length proceeding from all the different light-waves. But if by means of a lens we form an image of the light-source, so that each particular part shall be impressed in its proper place on the slit-plate, then in the spectrum the different kinds of light will be sorted out.

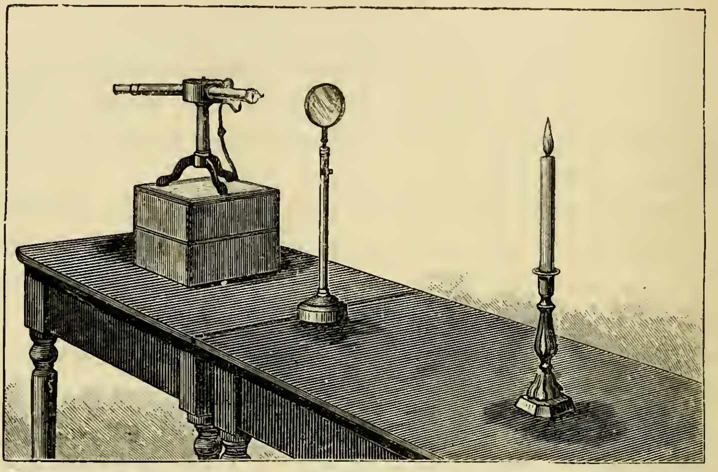

FIG. 59.—Arrangement for projecting an image of a candle flame on the slit-plate of a spectroscope.

There is a simple experiment which shows clearly the different results obtained. If we observe the light of a candle or a lamp with the spectroscope in the ordinary manner, that is by placing the candle in front of the slit at some little distance from it, we see a band of colour—a continuous spectrum; and in one particular part of the band we see a yellow line, and occasionally in the green and in the blue parts of the band other lines are observable. Now, if we throw an image of the candle or lamp on to the slit—the slit being horizontal and the image of the candle vertical—we then get three perfectly distinct spectra. We find that the interior of the flame, that is the blue part (best observed at the bottom), gives us one spectrum, the white part gives us another, while on the outside, so faint as to be almost invisible to the eye, there is a region which gives us a perfectly distinct spectrum consisting of a line in the yellow. In this way there is no difficulty whatever in determining the co-existence of three light-sources, each with its proper spectrum, in the light of a common candle or lamp.

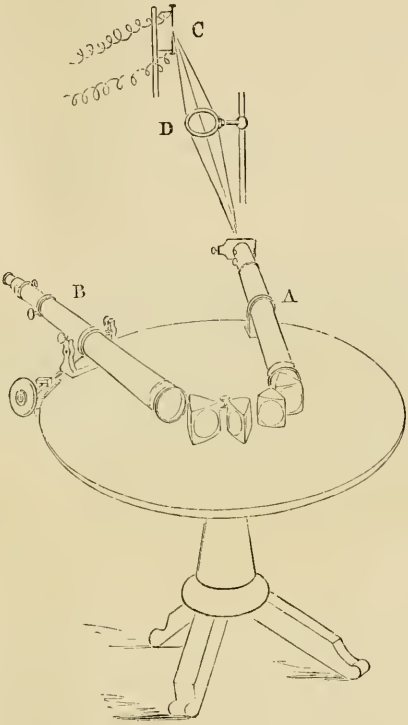

Fig. 60.—Showing arrangement of lens with large spectroscope. A, collimator; B, observing-telescope; C, spark; D, lens.

What a pity that Wollaston did not use such a lens in 1804!

We see in a moment that much the same condition of affairs will be brought about if, instead of using a candle, we use an electric arc or spark in which the pure vapour of the substance which is being rendered incandescent fills the whole interval between the poles, the number of particles being smaller and the degree of incandescence being less intense at the *sides* of the arc.

Take, for instance, the case of the volatilisation of iron in an electric arc. Since light from every part of the arc placed in front of a slit must enter every part of it, the differences between the light proceeding from the upper pole or the lower pole, or from the globule of iron which is being melted and exists in a liquid form, or from the vapours of iron which surround that liquid globule—if there are any such differences—are absolutely lost in observations made in the ordinary way. But if we introduce the lens between the light-source and the slit of the spectroscope; if, as we throw an image of the sun on the slit, we throw an image of the light-source on the slit, we ought to see any difference that may exist.

We readily do see that there are very considerable optical differences in the various parts of the image of the light-source. We have the upper and lower pole, the globule of iron volatilising, and the vapour, both in the arc, properly so-called, and the accompanying flame, each with its own special spectrum.

By an easily understood artifice we can throw an image of a *horizontal* arc on a *vertical* slit: the slit will give then the spectrum of a section of the arc at right angles to its length. The vapour which exists furthest from the core of the arc has a much more simple spectrum than that of the core of the arc itself, the spectrum of the core consisting of a large number of lines, all of which die out until the part of it furthest from the centre gives but one line.

It is obvious that if we throw the image of the electric arc on the slit in this manner we can examine the vapour without being inconvenienced by the bright continuous spectrum of the light from either of the carbon poles. It is also obvious that if we arrange the slit horizontally while the current is passing in a vertical direction from one pole to the other, we shall be able, by moving the slit upwards, to see if there are any differences observable in the vapour, first in the region where we have intense boiling and volatilisation going on, and in the necessarily cooler region where the arc is in contact with the outer air.

In this way the spectrum of each substance furnishes us with *long* and *short* lines, the long lines being common to the more and the less intensely heated parts of the arc, because we are not dealing with a simple section, and the short lines being confined to the more heated ones only.

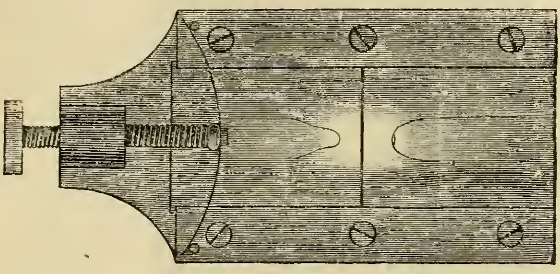

FIG. 61.—Arrangement for obtaining long and short lines. Image of the horizontal arc on slit-plate of spectroscope.

Whether we use the artifice of a horizontal arc with a vertical slit, or a vertical arc with a horizontal slit, does not matter, provided we keep the slit immersed in the light of the arc, and thus get rid of the light from the poles, and at the same time arrange the slit so that we can compare the light in the interior portion of the light-source with the light nearer its boundaries—if we take all these precautions we shall then get in the case of every substance such a result as shown in Fig. 62. We have in the centre a complete spectrum, its intensity being gradually toned down, and some of the lines being left behind as we look up and down towards the boundary where we have the spectrum of that portion of the arc which was the last to retain its luminosity in consequence of cooling.

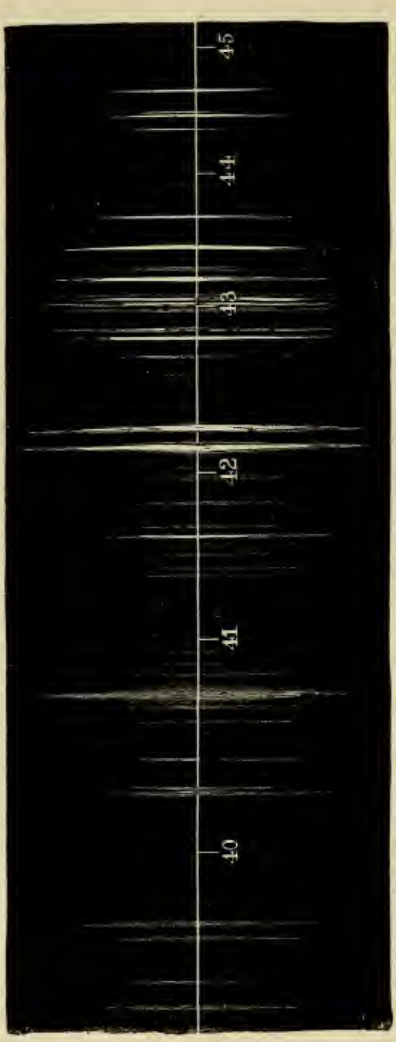

FIG. 62.—Long and short lines.

The illustration given is a copy of a photograph taken when a salt of strontium, strongly impregnated with calcium, is volatilised between the carbon poles in an electric lamp fixed in a horizontal position. The two longest lines, between w.l. 42 and 43, are due to strontium and calcium: the longest line, between 40 and 41, is due to strontium; the four short lines to the left are due to calcium and aluminium.

If we take horizons from the central portion of the diagram to the point most distant from that central axis, we find, in the case of every substance, the light at last becomes practically monochromatic. The vapour at this distance from the central axis radiates to us light spectroscopically represented by one line. As we get nearer and nearer the centre of agitation the spectrum becomes more complex, until at length, when very near the central axis, we get a great many short lines introduced, so that the spectrum at that point is most complex. This I am anxious to draw attention to with some insistence, because we shall understand at once the terms long and short lines, about which there will be a great deal in the sequel.

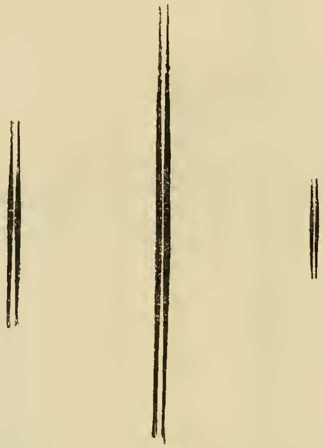

FIG. 63.—Spectrum of sodium, showing the long and short lines.

Figure 63 shows the much more simple spectrum of sodium. The longest line in the middle is D, that to the left, the line in the green, and we find that one set of the double lines excels all the others, and reaches a greater distance from the central axis.

An electric lamp can be arranged to show the long and short lines of sodium on a screen to a large audience. The arrangement is rather a delicate one, but the point is that we have not, as in the case of the electric lamp as ordinarily used, vertical

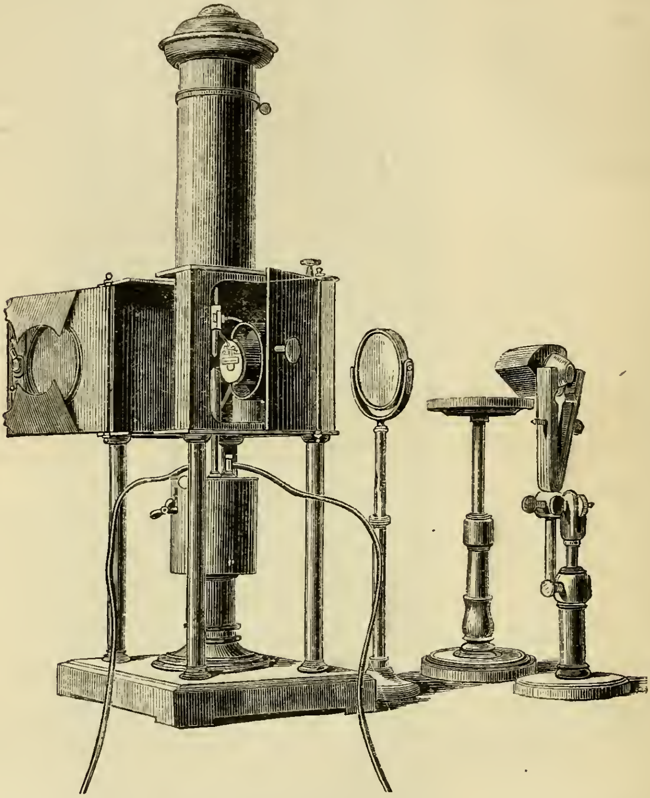

FIG. 64.—Another arrangement of apparatus for projecting the long and short lines on a screen, using a horizontal slit in the lamp close to the poles.

poles, but horizontal ones, and we have a vertical slit *close to the poles in the very middle of the lamp*, so that if the experiment is carried far enough we can then prove the accuracy of the statement that the line is an image of the slit, because the slit generally melts, and we see the shape of the lines varying on the screen as the melting of the slit goes on.

I began work on this method in 1869, but long before this other observers had obtained a glimpse of some of the phenomena, so obvious when it is employed.

Thus Dr. Gladstone, in 1860,[1] in the course of some experiments with the light produced by the passage of an electric current through an interrupted stream of mercury, remarked that "the more intense lines appeared broad on account of irradiation, *and of greater extent because the slightly luminous environment of the spark gives a perceptible amount of those rays.*"

This observation was followed up in 1862 by Professor Stokes,[2] who used the spark itself instead of a slit, and remarked that the metallic lines are "distinguished from air lines by being formed only at an almost insensible distance from the tips of the electrodes, whereas air lines would extend right across."

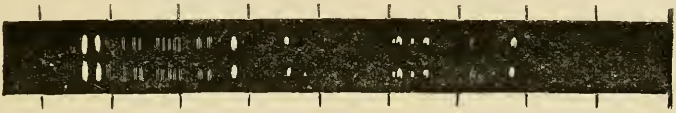

FIG. 65.—Copy of Dr. Miller's spectrum of cadmium, showing the "dots."

Miller,[3] who used a slit and a spark close to it, referring to his photographs of electric spectra, remarked, "The marginal extremities of the metallic lines leave a stronger image than their central portions," and the extremities of these interrupted lines he terms "dots."

The accompanying woodcut of the spectrum of cadmium, as photographed by him, will show the expressiveness of the word.

On the same subject Robinson[4] writes, "At that boundary of the spectrum which corresponds to the negative electrode (and in a much less degree at the positive) extremely intense lines are seen, . . . which, however, are short."

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[1] *Phil. Mag.* s. 4, vol. xx p. 250.

[2] *Philosophical Transactions*, vol. clii. 1862, p. 603.

[3] *Op. cit.* p. 877. *Op. cit.* p. 947.

Thalén observed this localisation to a certain extent, doubtless on account of the long collimator which he employed.[1]

In 1869, I observed that in a solar storm on the edge of the sun, when the magnesium lines were visible, the lines of that metal did not all attain the same height.

Thus of the *b* lines, b^1 and b^2 were of nearly equal height, but b^4 was much shorter.

Next I found that of the many iron lines observed by Ångström, only a very few were indicated in the spectrum of the chromosphere when iron vapour was injected into it from below.

Dr. Frankland and myself were enabled at once to connect these phenomena by reasoning that, as in the case of hydrogen, the spectrum became simpler and the lines thinner where the density was less.

To test the truth of this assumption by some laboratory experiments, we took the spark in air between two magnesium poles, so separated that the magnesium spectrum did not extend from pole to pole, but was visible only for a little distance, indicated by the atmosphere of magnesium vapour round each pole. We then threw an image of the magnesium pole and surrounding vapour on the slit of the spectroscope exactly as the sun and its surrounding vapours were thrown on the slit in the observatory.[2]

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[1] He remarks :—" Il y a aussi des raies brillantes qu'on n'observe que dans des cas exceptionnels, comme, par exemple, quand la quantité de la substance soumise à l'expérience est très-abondante ou quand l'incandescence devient très-vive. Ces raies qui se présentent ordinairement aux bords du spectre sous la forme de points d'aiguille, même quand les autres raies du métal forment des lignes continues à travers du spectre, ont été representées sur la planche par des lignes très-courtes." —*Mémoire sur la détermination des longueurs d'onde des raies métalliques*, p. 12, printed in the *Nova Acta Regiæ Societatis Scientiarum Upsaliensis*, ser. iii. vol. vi. Upsala, 1868.

[2] *Proc. Roy. Soc.* No. 115, 1869.

We then carefully examined the disappearance of the *b* lines *and found that they had behaved exactly as they do on the sun.* Of the three lines the most refrangible was the shortest. In fact, had the experiment been made in hydrogen instead of in air, the phenomena presented in the telescope would have been almost perfectly reproduced; for each increase in the temperature of the spark caused the magnesium vapour to extend further from the pole, and increased the disparity in the relative lengths of the lines.

I have given the above as the first experiment which indicated that this method of observation seemed likely to bring us much new knowledge, and indeed it will not be difficult to show that the results obtained by this method have a very important bearing upon every question connected with solar chemistry. When these spectra were observed—the spectra of the longs and shorts—of course we had a perfectly new set of phenomena to deal with. In all spectra observed on the old method all the lines had been practically seen of the same length, or else lengths shown in maps had represented intensities. But now we soon found that we had, in the case of each chemical substance, to deal with the remarkable fact that when the spectrum of that chemical substance was examined in this way, some of the lines were long and some of them short; and the question naturally arose, How is it that some of the lines are long and some of them short? That question was an exceedingly difficult one to answer. I do not know that it has been thoroughly answered yet. More than this, we wanted to know how the long and short lines behaved when the spectrum of any particular chemical substance was seen in the chromosphere.

We shall see that while researches were being made for the answer to these questions, certain general statements soon became possible which are of very considerable importance to us in our inquiry.

CHAPTER XII.

SOME RESULTS OF THE "LONG AND SHORT" METHOD.

1. *The Short Lines die out by mixing.*

In order to apply this new knowledge to the investigation of the chemistry of the sun, the first thing to be done was to prepare maps showing the long and short lines of those substances stated by Ångström, Kirchhoff and others, to exist in the solar atmosphere, and then to see if it was possible to *get a lead* by comparing these with the lines observed either in the general spectrum of the sun, or in the spectrum of any particular portion be it spot or prominence. This work was begun in 1870.

The following facts forced themselves at last upon the attention :—[1]

1. When a metallic vapour was subjected to admixture with another gas or vapour, or to reduced pressure, I found that its spectrum became simplified by the abstraction of the shortest lines and by the thinning of many of the remaining ones. To observe the effect of reduction of pressure, the metals were inclosed in tubes in which a partial vacuum was produced. *In all these experiments it was found that the longest lines invariably remained most persistently.*

2. When we used metals chemically combined with a metalloid—in other words, when we passed from a metal to one of its *salts* (I used the chlorides)—only the longest lines of the metal

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[1] *Phil. Trans.* 1873, p. 253 *et seq.*

remained. Their number was large in the case of elements of low atomic weight, and small in the case of elements of high atomic weight.

3. When we used metals mechanically mixed, of the smallest constituent, only the longest lines remained.

On this point I must enlarge somewhat by referring to a series of experiments recorded in the *Philosophical Transactions* (1873).[1] A quantity of the larger constituent, generally from five to ten grammes, was weighed out, the weighing being accurate to the fraction of a milligramme; and the requisite quantity of the smaller constituent was calculated to give, when combined, a mixture of a definite percentage composition by weight (this being more easily obtainable than a percentage composition by volume).

The quantities generally chosen were 10, 5, 1, and 0·1 per cent.

In a few cases, with metals known to have very delicate spectral reactions, a mixture of 0·01 per cent. was prepared.

Observations were then made of the spectrum of each specimen, and the result was recorded in maps in the following manner:—First, the pure spectrum of the smallest constituent was observed, and the lines laid down from Thalén's map.

The series thus mapped was as follows:—

Tin + Cadmium	percentages of	Cadmium	10, 5, 1, 0·15
Lead + Zinc	,,	Zinc	10, 5, 1, 0·1
Lead + Magnesium	,,	Magnesium	10, 1, 0·1, 0·01

The observations showed that the lines of the smallest constituent disappeared as the quantity got less. These observations then rendered possible such a general statement as this, for instance, that if we take, say, some iron, observe its spectrum, and then mix some manganese with it, and observe the spectrum of the mixture: if the quantity of manganese is very small, we shall only get the longest line of manganese; if the quantity of manganese is increased, the next longest line will come in; and so on. So that if the spectrum of any specimen of iron was observed, it was at once easy to see whether there was an impurity of manganese in that iron, if we made the admission that the spectra of iron and manganese, and so on, were the spectra of bodies not decomposable at the temperature which we were employing. If, for instance, there was a great quantity of manganese existing as an impurity in the iron, we got a great many lines, and of course with the quantity of admixture the number of lines would go on increasing until we might have fifty per cent. of each, when we should practically have the greatest number of lines of iron and the greatest number of lines of manganese we could ever get together. This was the *general* statement, but certain exceptions were noted.

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[1] Part ii. p. 479 *et seq.*

Although we had here the germs of a quantitative spectrum analysis, the germs only were present, because, from the existence of several "critical points," and great variations due to other causes, the results obtained were not constant.

In a subsequent research on the gold-copper alloys used in the coinage,[1] Mr. Roberts, the Chemist of the Mint, and myself were able to show that the shortening in the length of the lines by reduced quantity was such a definite physical effect that a difference of $\frac{1}{10000}$ part of copper in gold could be detected.

In the case of the metals, the electric spark was passed between metallic poles. For gases of course this method could not be employed. In practice there are very great objections to the use of Geissler tubes, one very valid objection being that the gas becomes much less luminous as its pressure is reduced; but in fact, the work connected with gaseous spectra can be done at atmospheric pressure; we can get the lines down,

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[1] *Phil. Trans.* 1873, Part ii. p. 495 *et seq.*

not by reducing the pressure, but by reducing the quantity of any particular gas in a mixture.

If we take, for instance, a spark in air and observe its spectrum, we find the lines of the constituents of atmospheric air considerably thick; but if we wish to observe accurately the lines of one of the constituents, say oxygen, these should be fine, in order to enable us to determine their absolute position. To accomplish this, the spark is taken in a glass vessel with two adits and one exit tube. If we wish to observe the oxygen lines fine, the vessel is flooded with nitrogen so that there is only a small quantity of oxygen present, and pass the current between the inclosed electrodes. If we wish to observe nitrogen lines fine, it is flooded with oxygen, so that there is only a small quantity of nitrogen present.

In this way, by merely making a mixture in which the gas to be observed is quantitatively reduced, so that the lines which we have to investigate are just visible in their thinnest state, we have a perfect means of doing it without any apparatus depending on the use of low pressures. A very great simplicity of work is thus introduced.

2. *The Long Lines are in some cases common to many Spectra.*

While on the one hand, as we have seen, the short lines are reduced in number with reduced quantity of the substance producing them, it soon became apparent on the other, that the long lines had a trick of making their appearance in many spectra. Any one who will consult Thalén's tables, or Kirchhoff's or Ångström's map, will note the many coincidences. This point, then, deserves careful study.

The following general statements were soon hazarded, with the proviso that it was possible that further inquiry might modify them.

1st. If the coincident lines of the metals are considered, those cases are rare in which the lines are of the first order of length in all the spectra to which they are common, especially if the volatility of the metals in question is about the same: those cases are much more common in which they are long in one spectrum and shorter in the others.

2nd. As a rule, in the instances of those lines of iron, cobalt, nickel, chromium, and manganese (substances volatilised with difficulty), which are coincident with lines of calcium (which volatilises easily), the calcium lines are long. Hence we are justified in assuming that lines seen in the spectra of iron, cobalt, nickel, chromium, and manganese, coincident with long and strong lines of calcium, are really due to traces of the latter metal occurring in the former as an impurity.

The question then naturally arose whether, in cases of coincidences of lines found between the lines of various spectra, the line may be fairly assumed to belong really to that one in which it is longest and brightest, and make its appearance merely as an impurity in all the others. Indeed, a prolonged examination of various spectra was not required to afford evidence not only of the great impurity of most of the metals used, but of the fact that many of the coincidences observed by Thalén and others might be explained without having recourse to the idea of physical coincidences.

A study of Ångström's map of the solar spectrum, to take an instance, shows many cases in which a line has been observed to be common to two or more spectra; and this is especially the case with the lines of iron, titanium, and calcium, nearly every other solar metallic spectrum exhibiting one or more cases of coincidence with the latter. In those cases which were examined in the light of the "long and short lines," it was frequently found that a line coincident in different spectra was long and bright in only one of them, and that in others it was short, or faint, or both; or even, in certain specimens of the substances, altogether absent from the spectrum.

As an instance of this difference of behaviour, the following cases in the spectra of calcium and strontium may be given.[1] The longest line in the visible portion of the calcium spectrum (Thalén), wave-length 4226·3, is found in the strontium spectrum as a line of medium length. 4607·5, one of the longest lines of strontium, appears in the calcium spectrum as a short line.

Another very long line of strontium occurs at 4215·3, in close proximity to the longest calcium line, and, according to Thalén, occurs also in the spectrum of that metal.

We have here, then, two metals with two lines common to their spectra; and it is found that the line which is long and bright in one spectrum, is faint in the other; and with regard to a third line, one observer finds it in both spectra, the other in one only, and after many attempts succeeds in observing it in the second, *but only in a specimen known to be contaminated with the first.*

The simplest explanation of the case, bearing in mind the facts already dwelt upon, is that the calcium used to produce the spectrum was contaminated to a certain extent with strontium, the strontium in turn containing calcium—a state of things which a moment's consideration will show to be not only possible but most probable, the close chemical relation of the two metals and the extreme difficulty of making even an approximate separation when mixed, being well known.

The long lines of calcium at wave-lengths 4226, 3968, and 3933, occur also in the spectrum of iron, cobalt, nickel, barium, and other metals, as observed in the arc *using carbon poles,* and assume very considerable proportions, equalling or surpassing in length many undoubted lines of those elements which are less easily volatilised by the action of the current; on the other hand, the iron lines at wave-lengths 4071, 4063, and 4045, occur in calcium, strontium, and barium, and in other metals under like conditions.

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[1] *Proc. Roy. Soc.* No. 147, 1873, p. 511.

Again, the longest lines of aluminium (wave-lengths 3961 and 3943) occur usually in the spectrum of iron as longish lines, and are to be found in the arc spectra of cobalt, nickel, calcium, strontium, barium and other metals, where they are even longer than some of the true lines of the metals in which they occur owing to their lower volatility.

It will be seen, then, that amongst the first questions raised was that of *impurities*, and it became clear that if this view were well founded, the revision of all cases of coincidences recorded by previous observers became absolutely necessary, since at last we had a method of eliminating impurities with certainty.

3. *Light thrown on the Fraunhofer Lines.*

There was another point in which our knowledge was at once increased in the most definite manner. Only those who knew least about solar matters took Kirchhoff's statement as to the identity of solar and terrestrial spectra *au pied de la lettre*; and Ångström had already pointed out discrepancies in the case of aluminium and zinc. Hence I attempted to see whether there was anything to help us in the simplification of spectra brought about by passing from the metal to the chloride. It was soon found that when we compared the spectra of metallic vapours reversed in the solar spectrum, such spectra being mapped by the new method, *i.e.*, showing the long and short lines, *the reversed lines in the spectrum were invariably those which are longest.* Here we had, in fact, a new test to apply to the reversal of solar lines, and a guide of the highest value in spectrum observations of the chromosphere and photosphere. It was seen at once that to the last published table of solar elements (that of Thalén) must be added zinc, aluminium, and possibly strontium, as a result of the application of the new test which it seemed worth while to pursue further.

In order to continue the inquiry under the best conditions, complete maps of the long and short lines of all the elements would have been of course necessary, but it was not absolutely essential for the purposes of a *preliminary* inquiry to wait for such a complete set of maps, since we had already learned that the lists of lines given by the various observers may be made to serve as a means of differentiating between the longest and shortest lines, because, as we have seen, the lines given at a low temperature, by a feeble percentage composition, or by a chemical combination of the vapour to be observed, are precisely those lines which appear longest when the complete spectrum of the pure dense vapour is studied.

Now with regard to the various lists and maps published by various observers, it was known (1) that very different temperatures were employed to produce the spectra, some investigators using the electric arc with great battery-power, others the induction-spark with and without the jar; (2) that some observers employed in certain cases the chlorides of the metals the spectra of which they were investigating, while others used the metals themselves.

It is obvious, then, that these differences of method could not fail to produce differences of result; and accordingly, in referring to various maps and tables of spectra, we find that some include large numbers of lines omitted by others. A reference to these tables, in connection with the methods employed, shows at once that the large lists are those of observers using great battery-power or metallic electrodes, the small ones those of observers using small battery-power or the chlorides. If the lists of the latter class of observers be taken, we shall have only the longest lines, while those omitted by them and given by the former class will be the shortest ones.

In cases, therefore, in which I had not then already (1873) mapped the spectrum in the new way, I in the first instance took the longest lines as thus approximately determined.

A preliminary search having been determined on, I endeavoured to get some guidance by seeing if there was any quality which differentiated the elements already traced in the sun from those not traced; lists were prepared showing broadly the chief chemical characteristics of the elements traced and not traced. This was done by taking a number of the best known compounds of each element (such, for instance, as those formed with oxygen, sulphur, chlorine, bromine, or hydrogen), stating after each whether the compounds in question were unstable or stable. Where any compound was known not to exist, that fact was indicated.

Two tables were thus obtained, one containing the solar, the other the more important non-solar elements (according to our knowledge at the time).

These tables gave me, as the differentiation sought, the fact that in the main the known solar elements formed stable oxygen-compounds. I have said in the main, because the differentiation was not absolute; but it was sufficiently strong to make me commence operations by searching among the Fraunhofer lines for the longest lines of the outstanding strong oxide-forming elements.

The immediate result[1] was that *strontium, cadmium, lead, copper, cerium,* and *uranium* were shown with considerable probability to exist in the solar atmosphere, in addition to the elements in Thalén's last list.

Certain of those elements which form unstable compounds with oxygen were also sought for; gold, silver, mercury, being examples. None of these were found, however.

This was in 1874. The total result then was as follows:—

Metals present in the Sun.

Sodium
Iron
Calcium
Magnesium
Chromium
Nickel
Cobalt
Hydrogen
Manganese
Titanium
Strontium
Cadmium
Lead
Copper
Cerium
Uranium.

[1] *Proc. Roy. Soc.* No. 147, 1873, p. 512.

4. *The Spectroscopic Effects produced by the Dissociation of known Compounds.*

There was another way in which the new method of work proved itself very valuable, though at first it did not seem to be connected in any way with solar chemistry.

With regard to this, I should commence by stating that from a beautiful series of researches carried on by several methods, Mitscherlich concluded, in 1864,[1] *that every compound of the first order heated to a temperature adequate for the production of light, which is not decomposed, exhibits a spectrum peculiar to this compound.*

In some experiments of my own, made in 1873, I observed:—[2]

First. That whether the spectra of iodides, bromides, &c., be observed in the flame or a weak spark, *only the longest lines of the metals are visible,* showing that only a small quantity of the simple metal is present as a result of partial dissociation, and that by increasing the temperature, and consequently the amount of dissociation, *the other lines of the metal appear in the order of their length with each rise of temperature.*

Secondly. I convinced myself that while *in air,* after the first application of heat, *the spectra and metallic lines are in the main the same, in hydrogen the spectra are different for each compound, and true metallic lines are represented according to the volatility of the compound, only the very longest lines being visible in the spectrum of the least volatile compound.*

Thirdly. I found that with a considerable elevation of temperature, *the spectrum of the compound faded almost into invisibility.*

These results enabled us to make the following statement:—

A compound body, such as a salt of calcium, has as definite a spectrum as that given by the so-called elements; but while the spectrum of the metallic element itself consists of lines, the number and thickness of some of which increase with increased quantity, the spectrum of the compound consists in the main of flutings and bands, which increase in like manner.

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[1] *Phil. Mag.* 1864, vol. 28, p. 176.

[2] *Phil. Trans.* 1873, pp. 650, 651.

In short, the molecules of a simple body and a compound one are affected in the same manner by an increase in their quantity in so far as their spectra are concerned; *in other words, both spectra have their long and short lines,* the lines in the spectrum of the element being represented in the spectrum of the compound by bands or fluted lines; and in each case the greatest simplicity of the spectrum depends upon the smallest quantity, and the greatest complexity upon the greatest.

The heat required to act upon such a compound as a salt of calcium, so as to render its spectrum visible, *dissociates the compound according to its volatility;* the number of true metallic lines which thus appear is a measure of the quantity of the metal resulting from the dissociation, *and as the metal lines increase in number, the compound bands thin out.*

CHAPTER XIII.

DIFFICULTIES.

At this point we must pause. The work referred to in the immediately preceding chapters, which brought us down to the end of the year 1873, was chiefly laboratory work. We must now return to the sun, and study not only the application of this new work to solar chemistry, but refer to some purely solar observations as well.

In this way we shall be able to note how the solar theories then in vogue stood the strain of the new tests we could apply to them; in short, we can inquire if the line was all clear for future progress.

We were working on two hypotheses or inferences more or less clearly enunciated by Kirchhoff.

I. The absorption which produced the Fraunhofer lines took place at some distance above the photosphere, the spots being solar clouds.

II. The chemical elements present in the solar atmosphere were identical with some of those existing on the earth, and their spectra were identical.

Was the way then perfectly clear, taking the work as it stood in 1873? Did work on the sun's atmosphere both during eclipses and independently of them, confirm Kirchhoff's views? And, moreover, did our chemical theories which Kirchhoff had taken as a base for his second hypothesis explain all the facts which had been gathered by many men in many lands touching solar chemistry? The inquiry had depended on using existing maps, whether tainted with impurities or not, and observing the lines in all prominences and spots.

I shall have to show that things were by no means clear; that both eclipse observations and others had not endorsed Kirchhoff's views, and that any one who took the trouble to bring together all the results which had been obtained up to that time would have found not only that there was a rift in the lute, but that there was a very big one; and that the discord which grew upon one as one went into details either with regard to the spectrum of the spots, to the spectrum of the prominences, or to the localisation of the solar atmosphere, was very much more remarkable than the accord. Of course, an immense amount had been done towards elaborating a view of solar chemistry a great part of which would stand; still there was a great deal which required a considerable amount of attention, and a great deal more which suggested that there was still a higher light to be got before we could really face the magnificent problem with which we were attempting to grapple.

In 1873 we had not only results garnered by the new method in six years to deal with, but we had the observations made in the eclipses of 1868, 1869, 1870, and 1871 to co-ordinate with them, and after such co-ordination to compare with the hypotheses in vogue.

With regard to eclipses it is difficult to overrate the value of the observations which they enable us to collect, for they increase our knowledge of the higher reaches of the sun's atmosphere which are inaccessible at any other times, and the lower layers can at the same time be seen under better conditions because there is less atmospheric light to contend with.

So much, indeed, has of late years been discovered by the new method touching both spots and prominences, that these are now regarded as but ordinary phenomena—we are familiar with them; and we are apt to forget the scale on which the changes rendered visible to us by our telescopes take place. This is not the case with the different class of phenomena which is revealed to us only during eclipses, when the moon shields the place of observation from ordinary sunlight and, by interposing herself exactly between us and the sun, allows us to inspect the sun's atmosphere with perfect ease. Then new glories are rendered visible which make the moments of the totality as precious to scientific men as they are terrible and awe-inspiring to ordinary beholders.

1. *The Solar Atmosphere.*

Let us, then, attempt to compare the totality of the knowledge thus acquired with that part of Kirchhoff's hypothesis which deals with the *locus* of absorption. The first year's work with the new method showed that the facts did not fit the theory, for the greatest widening of lines was always seen close to the photosphere; as a rule—though they were generally thinner than the Fraunhofer lines—they were thicker at the bottom than at the top. A diagram, Fig. 66, will show the importance of the last remark.

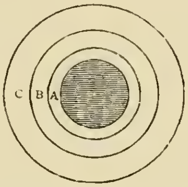

Fig. 66.—The sun and its atmosphere on Kirchhoff's hypothesis. A, luminous haze resting on liquid photosphere; B, locus of absorption; C, outer atmosphere.

If we imagine a slit of a spectroscope normal to the sun cutting the three layers, then the lines should be thickest in the spectrum of B according to Kirchhoff's view. But they were actually thickest at A.

It must never be forgotten (but it often is) that in our observations we are dealing with a sphere, and not with a section. So that if we imagine a layer to exist at any height above the sun, its spectral lines will be visible down to the spectrum of the sun itself. Whether, therefore, there is such a layer is very difficult of determination if we neglect to notice the various thicknesses of the lines. In fact the thickening towards the sun seems the only criterion ; if we suppose it not to exist, then it is easy to see that there is no spectroscopic method of determining the existence of the layers if we neglect temperature.

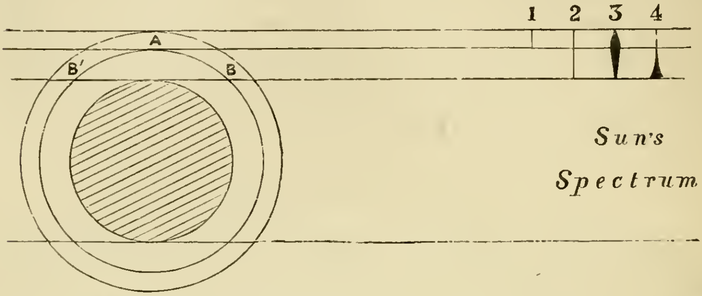

Fig. 67.—Diagram showing that if the sun's atmosphere consists of layers the lines will still extend down to the solar spectrum. For the layer A is also represented at B and B′, so that we shall see (2) in the spectrum and not (1). If thickness of stratum increased the width of the line, we should see (3) if the layer A did not extend to the sun, and (4) if it did.

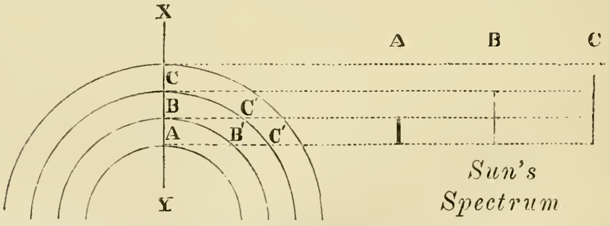

Fig. 68.—Spectral phenomena on the assumption of three layers. C, outermost layer, being represented at C′, will give us a long line as at C, as it would do if the stratum extended from C to A along the line X—Y, and so with B.

If we take temperature into consideration, then, as the lines will be less bright as the distance from the sun is increased, and therefore the temperature is reduced, *the longest lines will be the dimmest if they are produced by layers.*

The lines of A—the hottest layer—will be brightest and shortest.

The lines of B—the next cooler layer—will be less bright and longer, and will also appear to rest on the spectrum of the sun, as before explained, on account of the part of the layer at B′, although it is unrepresented at A, along the section X—Y.

And so on with C.

Now, chiefly because the lines increased both in width and number as we neared the photosphere, and also on account of other considerations, Dr. Frankland and myself soon came to the conclusion that in many particulars Kirchhoff's theory required modification.

In 1869 we wrote as follows [1] :—

"We believe that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received theory of the physical constitution of our central luminary,—the theory we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to this hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

"We find, however, instead of this compound atmosphere, one which gives us merely, or at all events mainly, the spectrum of hydrogen (it is not, however, composed necessarily of hydrogen alone, and this point is engaging our special attention); and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere such as the corona has been imagined to indicate, lies outside it; a view strengthened by the fact that the chromospheric bright lines present no appearance of absorption, and that its physical conditions are not statical.

"With regard to the photosphere itself, so far from being either a solid surface or a liquid ocean, that it is cloudy or gaseous, or both, follows, both from our observations and experiments. The separate prior observations of both of us have shown :—

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[1] *Proc. Roy. Soc.* vol. xvii. p. 288.

"1. That a gaseous condition of the photosphere is quite consistent with its continuous spectrum. The possibility of this condition has also been suggested by Messrs. De la Rue, Stewart, and Loewy.

"2. That the spectrum of the photosphere contains bright lines when the limb is observed, these bright lines indicating probably an outer shell of the photosphere of a gaseous nature.

"3. That a sun-spot is a region of greater absorption.

"4. That occasionally photospheric matter appears to be injected into the chromosphere. May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself, or extremely near to it, instead of in an extensive outer absorbing atmosphere? And is not this conclusion strengthened by the consideration that otherwise the newly discovered bright lines in the solar spectrum itself should be themselves reversed on Kirchhoff's theory? This, however, is not the case. We do not forget that the selective radiation of the chromosphere does not necessarily indicate the whole of its possible selective absorption; but our experiments lead us to believe that were any considerable quantity of metallic vapours present, their bright spectra would not be entirely invisible in all strata of the chromosphere."

Our view received, apparently, very strong confirmation afterwards. In my own observations occasionally in passing over a metallic prominence, the spectrum would be "full of lines"; and during the eclipse of 1870, at the moment of disappearance of the sun, the same effect was noticed; we had, to quote Prof. Young, "A sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality." The instrument used was an integrating spectroscope directed to the sun. In 1871 Captain Maclear and myself watched these lines on the retreating cusps. On these observations was based the view that there was a region some 2″ high above the photosphere which reversed for us *all* the lines visible in the solar spectrum; and on this ground the name "reversing layer" was given to it.

Still, undoubtedly some absorption was produced in the upper region of the solar atmosphere, for with each eclipse, against authority, against chemical arguments based upon the low atomic weight of the substances in the chromosphere, the corona grew and grew. In 1870 we had hydrogen 8′ from the sun, "far above any possible atmosphere;"[1] and in 1871 this was carried higher still; while towering above the hydrogen lines was another line, the famous 1474, which indicated something else existing in the exterior reaches of the atmosphere.

What, then, was the totality of the knowledge which had been acquired a few years ago with regard to the chemical nature of the sun's atmosphere taken as a whole—the sun's atmosphere from the upper reaches of the coronal atmosphere down to the region where, doubtless, the spot phenomena are located? The view of the sun's atmosphere, in 1873, was one something like this: We had, let us say, first of all an enormous shell of some gas, probably lighter than hydrogen, about which we know absolutely nothing, because so far none of it has been found here. Inside this we had a shell of hydrogen; inside this one of calcium, another of magnesium, another of sodium, and then a complex shell which has been called the reversing layer, in which we got all the metals of the iron group *plus* such other metals as cadmium, titanium, barium, and so on.

The solar atmosphere then, from top to bottom, consisted, it was imagined, of a series of shells, the shells being due *not to the outside substance existing only outside,* but to the outside substance extending to the bottom of the sun's atmosphere, and encountering in it, at a certain height, another shell which again found another shell inside it, and so on; so that the composition of the solar atmosphere as one went down into it got more and more complex; nothing was left behind, but a great many things were added. We had, dealing with known elements,

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[1] Young.

Highest . .	Hydrogen.
Medium . .	Magnesium, calcium, sodium.
Lowest . .	Iron, nickel, manganese, chromium, cobalt, barium, copper, zinc, titanium, and aluminium.

One word about the hydrogen.

We may have hydrogen, in a large globe, at such a pressure that we can make it luminous with a feeble current. If we examine it with the spectroscope we find it gives the F line alone, there is nothing red about it. Now there is a region around the sun which gives us something very like that in colour, and something absolutely like it, so far as the result of spectroscopic observation is concerned. Again, we may have hydrogen in a narrow tube in a condition to be considerably agitated; instead of allowing the current to act throughout a globe, it has to pass through a fine capillary space in which the gas is confined. That is a condition which is supposed to give us the effect of high temperature. It really does give us something like what we see in the next lower solar region. As we pass from few encounters of molecules to many, the gas is very much more luminous, and it is red. The level which gives such a spectrum as is got from the capillary tube is considerably lower than the one which gives us the F line alone (Fig. 69).

We may go more into detail with regard to the lower reaches. Further down, as has been already pointed out, we got all round the sun at certain periods of the solar activity some lines seen in the spectrum of magnesium. Underneath this again we got a layer in which lines seen in the case of sodium and calcium are almost as constantly seen. Still a lower depth—

practically there is no end of them—in which we got the lines of iron and other substances. There are many lower variable layers depending upon local disturbance. We have by these observations a means of determining the fact that the solar atmosphere consists of what may be very conveniently and justly called a very considerable number of layers; and what happens with these layers is this: If the sun is quiet, or if we observe any particular part of it at any time at which it is not agitated, the layers visible at that time, few in number, are nearly concentric (Fig. 69), but the moment there is any agitation in the subjacent photosphere the lower layer shoots up into the next layer above it; the next shoots up into the one next above that; and so on (Fig. 70). How far into the very confines of the solar atmosphere this sort of action goes we do not know, because it wants more time to observe than is afforded by an eclipse, but it is certainly known that from the very lowest layer to the upper hydrogen one the layers are made to obey this same impulse, and bulge up like so many domes on that part of the sun which is being violently agitated.

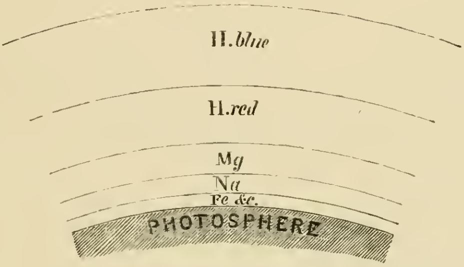

FIG. 69.—Imagined stratification of the solar atmosphere (1873).
(H = Hydrogen; Mg = Magnesium; Na = Sodium; Fe = Iron.)

The following extract from a lecture I gave at Cambridge in 1871 refers to an experiment of some interest in this connection:—

"It is easy to show also that with increased pressure comes such an increased brilliancy as we get in the sun, where, going inwards, we have first the faint corona, then the hydrogen layers, visible only in an eclipse, then those observed every day by the new method, and last of all, the photosphere itself.

"There is a simple experiment bearing upon this point. If we take a tube containing hydrogen at a very low pressure, and at the bottom of it some mercury, so long as the mercury is cool the spark passes through nearly pure hydrogen, and the tube is lighted up with only a faint glimmer, the equivalent of the auroral discharge.

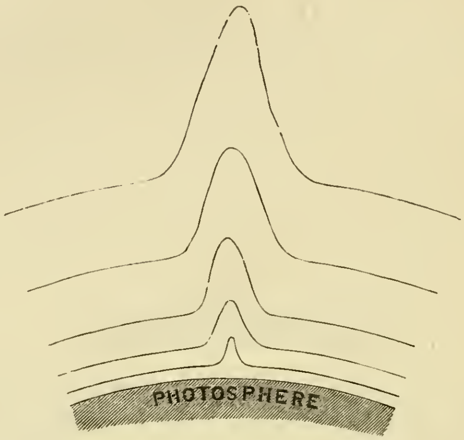

FIG. 70.—Stratification of the solar atmosphere disturbed by the upheaval of a prominence.

"But if instead of having our mercury vapour almost absent from the tube, in consequence of the low temperature of the liquid supply at the bottom, we drive this liquid mercury at the bottom of the tube into a state of vapour, we find that we not only change the colour of the discharge in the lower regions of the tube, but imitate what we get on the sun itself. As the discharge passes through the denser layers, and renders them incandescent as they are at the sun, the brilliancy increases enormously as the source of the vapour is approached, and in each stratum of different density of the mixed gas and vapour in the tube we have the same increase of brightness as we observe in the similar strata of the sun's atmosphere."[1]

So much for the first results obtained in localising the solar chemistry. We pass from a general theory, that the absorption is above the sun, and that the sun consists of such and such chemical substances, to a very much more complete picture, in which we see that the solar absorption is built up by vapours extending to certain definite heights in the sun's atmosphere, and that they are ever changing their heights and distribution at different times.

It should be seen clearly, from the foregoing statements, how wide, after the accumulation of these facts, was the chasm which separated Kirchhoff's view of the construction of the solar atmosphere from the facts as we imagined them in 1873.

There is, indeed, another point to which a slight reference may be made. It is impossible on Kirchhoff's hypothesis to explain the existence of bright lines in the ordinary solar spectrum, and yet that such bright lines do exist is undoubted. They were first noticed in 1869, when a large dispersion was employed, and they have been re-discovered two or three times since.

The bright lines seen on the disc of the sun also from time to time, which have been previously referred to while discussing the phenomena of the spots, also militate against this hypothesis.

2. *The Chemical Nature of the Sun unlike that of the Earth.*

Let us now leave the distribution of the chemical substances, and deal with the atmosphere of the sun taken as a whole. This is the only way open to us if we wish to consider the chemical structure of the sun itself. If the elements were identical we should not be justified in imagining any great departure from the terrestrial distribution. But here again the facts were all the other way.

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[1] Rede Lecture, Cambridge, 1871.

How came it that the total chemical composition of the atmosphere of the sun, which we were taught to look upon as the exemplar of what must have once existed in the case of our own planet, varied so enormously from the composition of the crust of our earth?

Among all the metalloids known to chemists only one of them—or one substance classed as such, hydrogen—was present in the solar atmosphere, and that in overwhelming quantity; whereas the efforts of Ångström, Kirchhoff, and others, could not trace such substances as oxygen, chlorine, silicon, and other common metalloidal constituents of the earth's crust. It was difficult to imagine a stronger difference to exist between any two masses of matter than that found between the incandescent sun, and the earth which is now cooling.

Even the elements in the sun were in an unnatural order according to the received chemical views. To give an instance:—The layer produced by what was taken to be gaseous magnesium round the sun, a layer indicated by the brightest member of the *b* group, was always higher—always gave us longer lines—than that other layer which was brought under our ken by the bright line D seen in the spectrum of sodium. Here was a distinct inversion of the chemical order. The atomic weight of sodium being 23, and of magnesium 24, the sodium ought to have been higher than the magnesium; but the contrary was the fact, and that fact has been established by twelve years of observation.

3. *The Chemical Nature of the Sun unlike that of many of the Stars.*

If then the sun and earth were chemically unlike, it was of the highest importance to learn how the facts stood in the case of the stars—a point of view very soon forced upon solar observers by the success which had attended the labours of Rutherfurd, Miller and Huggins, Secchi, and other observers in recording the spectra of stars.

This most interesting inquiry naturally enabled us to know whether the stars gave spectra quite like the sun, and if it happened that they did not give spectra like the sun or each other, then such a result would not only define the points of difference but would be sure to give us some excellent working suggestions.

How striking the differences actually were, not only between sun and star, but between star and star, will be shown in the sequel.

4. *Divergences between the Lines in the Spectra of Vapours and the Fraunhofer Lines.*

Kirchhoff in his memorable paper of 1861, which may justly be regarded as the basis of all subsequent work, was careful to state that the sixty iron lines in the sun to which he referred,[1] agreed, "*as a rule*," in intensity with those observed in the electric spark. Those who have given an account of his work have not always been so cautious. Indeed, I find my friend Professor Roscoe [2] running beyond the record in the following sentence :—

> "In order to map and determine the positions of the bright lines found in the electric spectra of the various metals, Kirchhoff, as I have already stated, employed the dark lines in the solar spectrum as his guides. Judge of his astonishment when he observed that dark solar lines occur in positions connected with those of all the bright iron lines! Exactly as the sodium lines were identical with Fraunhofer's lines, so for each of the iron lines, of which Kirchhoff and Ångström have mapped no less than 460, a dark solar line was seen to correspond. Not only had each line its dark representative

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[1] *Researches on the Solar Spectrum and the Spectra of the Chemical Elements*, Roscoe's translation, part i. p. 19.

[2] *Spectrum Analysis*, 3rd edit. p. 240.

in the solar spectrum, *but the breadth and degree of shade of the two sets of lines were seen to agree in the most perfect manner, the brightest iron lines corresponding to the darkest solar lines.*"

This statement (the italics are mine) was made to prove the absolutely identical nature of the iron vapour in the sun's atmosphere and in the electric spark.

Minute study however revealed the fact that the intensities of the various lines really vary very greatly from vapour to sun.

I wrote in 187 :—[1]

> "It is obvious that greater attention will have to be given to the precise *character* as well as to the position of each of the Fraunhofer lines, in the thickness of which I have already observed several anomalies. I may refer more particularly at present to the two H lines 3933 and 3968 belonging to calcium, which are much thicker in all photographs of the solar spectrum" (I might have added that they were by far the thickest lines in the solar spectrum) "than the longest calcium line of this region (4226·3), this latter being invariably thicker than the H lines in all photographs of the calcium spectrum, and remaining, moreover, visible in the spectrum of substances containing calcium in such small quantities as not to show any traces of the H lines.
>
> "How far this and similar variations between photographic records and the solar spectrum are due to causes incident to the photographic record itself, *or to variations of the intensities of the various molecular vibrations under solar and terrestrial conditions,* are questions which up to the present time I have been unable to discuss."

The progress of the work showed that the differences here indicated are not exceptions, but are truly typical when the minute anatomy of the solar spectrum is studied.

5. *Divergences between the Spectra of Vapours and the Lines visible in Spots and Prominences.*

We have dealt as yet with the spectroscopic difficulties so far as the ordinary spectrum of the sun is concerned, but in 1874 a great advance had been made in the localization of solar phenomena; in short, there was now another kind of work—a newer kind of work—going on. Observers began to give attention to the bright lines of solar prominences, and the lines thickened in solar spots. I shall here limit myself to the general statement that the divergence between the spectra of the different substances as observed in the sun and in our laboratories was very much intensified as facts were accumulated from these new regions. Very many of the lines observed in prominences were lines with no terrestrial equivalents, and the spot-spectrum often contained lines much thickened which were either not represented at all, or only feebly, among the Fraunhofer lines.

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[1] *Phil. Trans.* vol. clxiv. part ii. p. 807.

As the work of tabulating the lines went on, and the more complex outpourings of vapours from the sun's interior were studied, it was found that the prominence lines coincident with those seen in the spectra of iron, calcium, and so forth, were by no means the brightest lines—by no means the most important or most prominent lines—in the known spectra of those substances, but lines which really we had very great difficulty in recognising as characteristic of any particular spectrum. There they certainly were, however, mapped as very fine lines by the most industrious observers. Similarly with the spots, there was an absolute inversion of the thicknesses of the lines of any one substance.

6. *Motions indicated by Different Lines in Spots and Flames.*

Closely allied to these results we had another extraordinary fact. We could quite understand why in a spot the change of refrangibility of the magnesium lines when there was a storm going on in the sun should be different from the change of refrangibility of, say, the iron lines. The natural explanation was, of course, that the magnesium gas was going at one rate, and the iron gas at another rate. But it was soon found that the differences which could be sharply seen between the spectrum of a particular mass of magnesium vapour and a particular mass of iron vapour extended to the iron vapour itself.

There were just as many variations in the refrangibility of the lines of iron itself, for instance, as there were between the lines of iron and of other substances: that is to say, we had in the one case magnesium going at one rate and iron going at another rate; but when we came to deal with the iron lines alone we found one iron line told us the iron vapour was going at one rate, and another iron line told us that the iron vapour was going at another rate, *i.e.* a thing which could not be divided was going at two rates at the same time.

Further. The lines on which these determinations of the relative motions of the vapour depended were found to go in sets. In a spot, for instance, we would generally see movement indicated by one set of iron lines, whereas in a prominence we would always see a different set—a set in a different part of the spectrum altogether—registering this movement for us. Here again was considerable food for thought.

That was stated very roundly a good many years ago—in 1869. I will quote what I then wrote on this subject:[1]—"Alterations of wave-length have been detected in the sodium, magnesium, and iron lines in a spot spectrum. In the case of the last substance, the lines in which the alteration was detected were *not* those observed when iron (if we accept them to be due to iron alone), is injected into the chromosphere."

That caveat with regard to iron arose from the fact that of the 460 lines recorded, only three lines had up to that time been seen bright in the solar prominences.

It will, I think, be clearly gathered from the foregoing, that the more observations were accumulated the more the spectroscopic difficulties increased.

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[1] *Proc. Roy. Soc.* No. 115, 1869.

7. *Spectral Lines common to two or more Spectra.*

We now pass to difficulties of another order met with in the work of comparing the lines of the different elementary bodies with the Fraunhofer lines—work done chiefly in the first instance by Kirchhoff, Ångström, and Thalén. Kirchhoff was not long before he found that to say that each substance had a spectrum entirely and specially belonging to that particular substance was not true. He says,[1] "If we compare the spectra of the different metals with each other, several of the bright lines appear to coincide." Now Kirchhoff was working with Bunsen as his collaborateur, and therefore, as we may imagine, this was not said lightly. Similarly Ångström, who was working with the assistance of the Professor of Chemistry at Upsala, was driven to exactly the same conclusion. He says,[2]—I translate his words—"Of all bodies iron has certainly produced the greatest number of lines in the solar spectrum. . . . Some of these seem to be common with those of calcium." Thalén carried on this inquiry, and if one compares the magnificent tables which we owe to his untiring skill and industry, one is perfectly astonished to find the number of coincidences which he has so carefully tabulated. It might be imagined naturally that these lines were due to feeble dispersion or to impurities, but there was no spectroscopic method then of establishing the latter assumption.

8. *Spectral Lines vary their Intensities by Temperature.*

We now come to the discrepancies between the spectra on varying the temperature which soon forced themselves upon the attention of observers.

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[1] *Researches on the Solar Spectrum and the Spectra of the Chemical Elements*, Roscoe's translation, p. 10.

[2] *Recherches sur le Spectre Solaire*, p. 36.

Ångström, in that exceedingly important memoir which accompanies his Atlas, states :—[1]

> "In increasing successively the temperature I have found that the lines of the spectra vary in intensity in an exceedingly complicated way, and consequently new lines even may present themselves if the temperature is raised sufficiently high."

Kirchhoff and Bunsen, indeed, as early as 1860, seem to have got a glimpse of the same thing. They wrote :—[2]

> "If the intensity of the light . . . be increased new lines appear . . . and the relation of the brightness of the old ones becomes altered. In general an indistinct line becomes brighter, upon increasing the illumination, more rapidly than does a brighter line, but not to such an extent that the indistinct line ever overtakes in intensity the brighter one. A good example of this is seen in the two lithium lines. We have only observed one exception to this rule, namely in the line Ba η, which by light of small intensity is scarcely visible, whilst Ba γ appears plainly, but by light of greater intensity becomes more visible than the latter."

Touching these variations I may remark that Kirchhoff did not agree that the temperature upon which Ångström laid such strong stress was really the cause at work.[3] He attributed those variations rather to the mass and the thickness of the vapours experimented upon—nay, he went further: at a time when scarcely any facts were at his command he broached a theorem which went to prove this; and yet what had Kirchhoff himself done? He had traversed his own theorem. He states that his observations (those referred to in Chap. VI.), were made by means of a coil, using iron poles one millimetre in thickness. Now the thickness of a short spark taken from iron poles one millimetre in thickness would probably be two millimetres. Next Kirchhoff located the region where the absorption which produces the reversal of the iron lines takes place at a considerable height in the atmosphere of the sun, and he imagined the atmosphere of the sun to be an enormous mass represented by the old drawings of coronas, so that on his view the thickness of the iron vapour which reversed the iron spectrum must have been, at a moderate estimate, 10,000 miles, and yet he said that the spectrum of that, and of the light given by the electric spark two millimetres in thickness, were absolutely identical; that is to say, that the *fact* was that the variation of thickness from two millimetres to 10,000 miles made no difference. That was on the one hand; on the other hand he gave us his *theorem*, showing that a slight variation of thickness would produce all the changes which Ångström and others had observed up to that time, and which, it may be added, have been observed since in still greater number.

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[1] *Recherches sur le Spectre Solaire*, pp. 38, 39.

[2] Kirchhoff and Bunsen, *Phil. Mag.* vol. xx. s. 4, p. 94 August, 1860.

[3] *Recherches sur le Spectre Solaire*, pp. 38, 39.

We now know that in the case of hydrogen a difference in thickness of a million miles and of a millimetre makes no difference.

9. *The same Substance may have more than one Spectrum.*

At the time that Kirchhoff announced the conclusion that the terrestrial elements as known to chemists existed in the sun, the general idea was that each element had only one spectrum, and that that spectrum was the same whether observed in the sun or in our laboratories. Underlying these general notions was, as I have before stated, the assumption that the "chemical atom," a thing with a definite weight given in all chemical text-books, once got, even the solar temperature was insufficient to simplify it.

Soon after Kirchhoff had published his papers, three eminent Germans—Plücker and Hittorf[1] and the younger Mitscherlich—found that in the case of a great many simple substances, what are called *fluted spectra*, as well as line spectra, were to be observed.

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[1] *Phil. Trans.* 1865, vol. clv. pp. 1-29.

The accompanying diagram (Fig 71) of the fluted spectrum of iodine will show the difference between these fluted spectra and the line spectra on which we have been exclusively occupied up to the present.

We observe that the chief novelty is an absolute rhythm in the spectrum; instead of lines irregularly distributed over the spectrum, we have groups which are beautifully regular in their structure. The next diagram (Fig 72) shows us the radiation spectrum of a particular molecular grouping of carbon vapour; that also is beautifully rhythmic, the rhythm of each of the elementary flutings strongly resembling that of iodine.

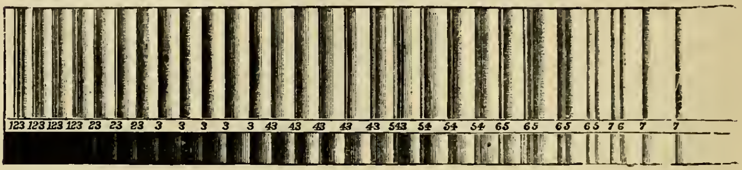

FIG. 71.—Fluted spectrum of iodine. (Thalén.)

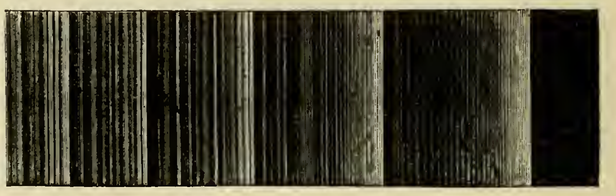

FIG 72.—Carbon flutings.

These observations were among the first to suggest the idea that the same chemical element could have two completely distinct spectra. They were eminently suggestive, for if two, why not many?

In the reference to the "long and short" method of observation it was stated that it enabled us to note what happens when a known compound body is decomposed. With ordinary compounds, such as chloride of calcium and so on, one can watch the precise moment at which the compound is broken up—when the calcium begins to come out; and we can then determine the relative amount of dissociation by the number and brightness of the lines of calcium which are produced. Similarly with regard to these flutings we can take iodine vapour, which gives us a fluted spectrum, and we can then increase the temperature suddenly, in which case we no longer get the fluted spectrum at all; or we may increase it so gently that the true lines of iodine come out one by one in exactly the same way that the lines of calcium become visible in the spectrum of the chloride of calcium. We end by destroying the compound of calcium and its spectrum in the one case, and by destroying the fluted spectrum of iodine in the other, leaving, as the result in both cases, the bright lines of the constituents—in the one case calcium and chlorine: in the other case iodine itself.

I have by no means exhausted the list of difficulties which were gradually presented to us when we considered that both in the sun and in our laboratories spectrum analysis brought before us phenomena due to the vibrations of unique, absolutely similar "chemical atoms." Not only were there differences, but the differences worked in different ways, whether we passed from low to high temperatures in laboratory work, or from the general spectrum to the prominence- or spot-spectrum in the sun.

But I have said enough for my present purpose; details on some points I have referred to must be gone into in the next chapter.

CHAPTER XIV.

DETAILS OF SOME OF THE DIFFICULTIES.

1. *The Spectrum of the Prominences.*

During the accumulation of the observations of the bright chromospheric lines described in Chapter IX., many phenomena difficult to understand presented themselves.

The following is a list[1] of the lines the positions of which were known at the end of 1869 :—

Kirchhoff's numbers.	Wave-lengths.	Element in the spectrum, of which corresponding lines had been recorded.
C	6561·8	Hydrogen.
F	4860·6	,,
Near G	4340·1	,,
h	4101·2	,,
Near D	5874·9	(?)
D	5895·0 5889·0	Sodium.
1989·5	4933·4	Barium.
2031·2	4899·3	,,
b_1	5183·0	Magnesium.
b_2	5172·0	,,
b_3	5168·3	Nickel.
b_4	5166·7	Magnesium.
1474	5315·9	Iron.
1515·5	5275·0	(?)
1529·5	5263·9	Bright line.

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[1] "Spectroscopic Observations of the Sun," No. V., *Proc. Roy. Soc.* vol. xvii. pp. 74 and 118, July, 1869.

Kirchhoff's numbers.	Wave-lengths.	Element in the spectrum, of which corresponding lines had been recorded.
1567·5	5233·6	(?)
1613·8	5197·9	(?)
1867·0	5017.6	Iron.
1871·5	5014·8	Bright line.
2001·5	4923·1	Iron.
2003·4	4921·3	(?)
2054·0	4880·5	(?) Band or line near black line, very delicate.

When this list was communicated to the Royal Society I remarked :—

" I refrain from dwelling on this list at present, except to point out that *taking iron as an instance, and assuming that the iron lines mapped by Ångström and Kirchhoff are due to iron only, I have only been able up to the present time to detect three lines out of the total number* (460) in the spectrum of the lower regions of the chromosphere—a fact full of promise as regards the results of future laboratory work. The same remark applies to magnesium and barium."

It will be seen at a glance how variously the known chemical substances were spectroscopically represented, and what a large percentage of lines there was about which we knew absolutely nothing.

It is true we had all the then known lines of hydrogen, but sodium was only represented by 2 lines out of 9; magnesium by 3 out of 7; barium by 2 out of 26; and iron by 3 out of 460!

Further, while the lines of sodium and magnesium were the brightest in the spectra of those elements, the lines of the other known bodies were not.[1]

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[1] The two barium lines contained in my list are the two strongest barium lines in the F to *b* region, but are not the strongest barium lines. The line of nickel seen is one of the brightest nickel lines visible in the arc, but not the brightest seen in the spark, as mapped by Thalén.

Nor was this all; some of the bright lines in the spectrum of the photosphere itself were continued into the prominence spectrum. Strange and unexpected as these observations were, they were soon endorsed. Thus Vogel gave the following list.[1]

	Wave-length.	
C	6561·8	Hydrogen.
D_3	5874·3	
D_1	5895·0	Sodium.
D_2	5889·0	,,
	5315·5	Iron (?)
b_1	5183·0	Magnesium.
b_2	5172·0	,,
b_3	5168·0	Nickel.
b_4	5167·0	Magnesium.
	5017·6	Iron.
	4923·1	,,
F	4860·6	Hydrogen.
	4340·0	,,

About the same time Lorenzoni directed his attention to this subject, and not only indicated several new lines, but gave frequencies of appearance. His new lines, like some of the old ones, had no counterparts among the Fraunhofer lines; notably one, which he indicated by *f*, at wave-length 4471·2, and another between B and C.

In twenty-six protuberances seen in one month we have *f* appearing 19 times against hydrogen 26; and the unknown substance which produces D^3 26 times against magnesium 8 and sodium 6. Three other lines, one between B and C, and two between F and *b*, were only seen once.

Fortunately, Lorenzoni was not the only one who was utilising the clear sky of Italy for this work.

Tacchini saw a great number of lines brightened in his observations of prominences extending from 1871 to 1873.

[1] H. C. Vogel, *Beobachtungen*, 1872, pp. 36, 37.

[2] *Memoire della Societa degli spettroscopisti Italiani*, vol. i. 1872.

Especially noticeable were two lines at w.l. 4923·1, 5017·6. given in my list, which were seen with great frequency till 1873, when they disappeared, and two new lines, about which nothing is even yet known, came in their place.[1]

About this time an American astronomer, Professor Young, made an important series of observations during an expedition to Sherman, a point 8,000 feet high in the Rocky Mountains. He saw an immense number of bright lines in the prominences, but a study of them only served to show our ignorance. Thus the longest iron line in the region between F and *b* was only seen once, while a much fainter iron line at 4923·1 was seen forty times. Another iron line at 4918·2 was seen twenty times, while an equally strong line at 4919·8 was not seen at all. In my observations I only saw three iron lines out of 460, and this, as I have shown, was confirmed by Vogel and Tacchini. Young, with much greater advantages as regards climate, was able to see 110 lines. He also saw the H and K calcium lines with a frequency of seventy-five and fifty respectively, while the much stronger blue calcium line at w.l. 4226·3 was seen only three times.[2] In the reduction of these observations he pointed out the fact that the lines which he had seen most frequently were lines common to two or more elements. He writes :—[3]

> "Two explanations suggest themselves. The first, which seems rather the more probable is, that the metals operated upon by the observer who mapped their spectra were not absolutely pure—either the iron contained traces of calcium and titanium, or *vice versâ*. If this supposition is excluded, then we seem to be driven to the conclusion that there is some such similarity between the molecules of the different metals as renders them susceptible of certain synchronous periods of vibrations—a resemblance, as regards the manner in which the molecules are built up out of the

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[1] Tacchini, *Memoire della Societa degli spettroscopisti Italiani*, vol. i. p. 89; vol. ii. pp. 55, 59, 60, 62, 95; vol. iii. p. 95; vol. iv. pp. 81 *et seq.*

[2] Young, *United States Coast Survey Report*, 1872.

[3] *Nature*, vol. vii. p. 17 *et seq.*

constituent atoms, sufficient to establish between them an important physical (and probably chemical) relationship."

If I understand Professor Young aright, his last words refer to what have been generally termed physical coincidences, that is, cases in which the common lines, being proved not to be due to impurity, are ascribed to common vibrations of dissimilar molecules.

It will thus be seen that the further the work was pushed the greater became the difficulty, and it will, I think, be gathered that in these observations of the lines visible in the sun's chromosphere, by the new method, the idea that we witnessed in solar storms the ejection from the photosphere of vapours of metallic elements *with which we are familiar on the earth* became more and more improbable.

The work, in short, of which I have given only the germs, revealed the most striking anomalies; nevertheless, loyal to the old views, we have all of us continued to talk of "injections of iron into the chromosphere," "magnesium prominences," and the like.

2. *Phenomena presented by the Stars.*

Since the sun is after all but a star—the nearest star to us, it is of primary importance for our purpose that the spectra of the more distant ones should be compared with it. This is now, in part, possible, for although it was Fraunhofer who at the beginning of the century laid the foundations of a science which we may christen Comparative Stellar Chemistry, it is only in our own day that the work has been begun in earnest by Rutherfurd, Miller, Huggins, Secchi, Vogel, and others.

Dealing with the knowledge already acquired, in 1873, along this line, we may say roughly that there were four genera of stars recognisable by their spectra.

We have first the brightest and presumably hottest stars, and of these the spectrum is marvellously simple—so simple, in fact, that we say their atmospheres consist in the main of only a very few substances—a statement founded on the observation that the lines in their spectra are matched by lines which we see in the spectra of hydrogen, magnesium, and perhaps of sodium too, but the faintness of the indication of these two latter substances only intensifies the unmistakable development of the phenomena by which the existence of the former is indicated.

So much for the first class: now for the second. In this we find our sun. In the spectra of stars of this class, the indications of hydrogen are distinctly enfeebled, and, accompanying this change, we find all simplicity vanished from the spectrum. The sodium and magnesium indications have increased, and a spectrum in which the lines obviously visible may be counted on the fingers is replaced by one of terrific complexity.

The complexity which we meet with in passing from the first class to the second is one brought about by the addition of the lines produced by bodies of chemical substances of moderate atomic weight. The additional complexity observed when we pass from the second stage to the third is brought about by the addition of lines due in the main to bodies of higher atomic weight. And—this is a point of the highest importance—at the third stage the hydrogen, which existed in such abundance in stars of the first class, has now disappeared.

Dr. Huggins' drawings of the spectra of Aldebaran and *a* Orionis (Fig. 73) show how much care has been devoted to this inquiry. Below the stellar spectra are mapped the lines of the various elements with which they were compared, and from the coincidence of some of these lines with those in the stellar spectra the existence of the corresponding elements in the atmospheres of these stars was inferred.

It will be seen that in the spectrum of Aldebaran, the hydrogen lines C and F are present, while they are absent from *a* Orionis. No less than seventy lines have been observed in the spectra of these two stars, and Mr. Huggins and Dr. Miller have detected in Aldebaran the following elements:—Hydrogen, sodium,

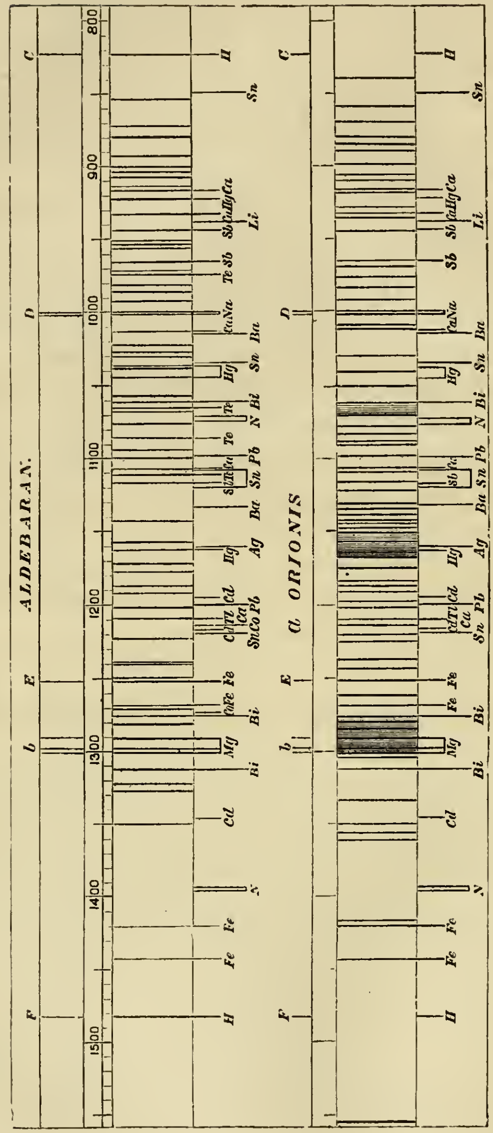

FIG. 73.—Huggins' stellar spectra.

[Ag = Silver. Ba = Barium. Bi = Bismuth. Ca = Calcium. Cd = Cadmium. Co = Cobalt.
Hg = Mercury. Na = Sodium. Pb = Lead. Sb = Antimony. Sn = Tin. Te = Tellurium.]

magnesium, calcium, iron, bismuth, tellurium, antimony, and mercury.

In the last class of stars to which I have referred, the fourth, the lines have given place to fluted bands, at the same time that the light and colour of the star indicate that we have almost reached the stage of extinction.

Here are some typical stellar spectra (Fig. 74), which show us at once the very considerable difference in the phenomena observed. In the upper part of this diagram we have a star, of Type I, remarkable for the fewness of lines in its spectrum.

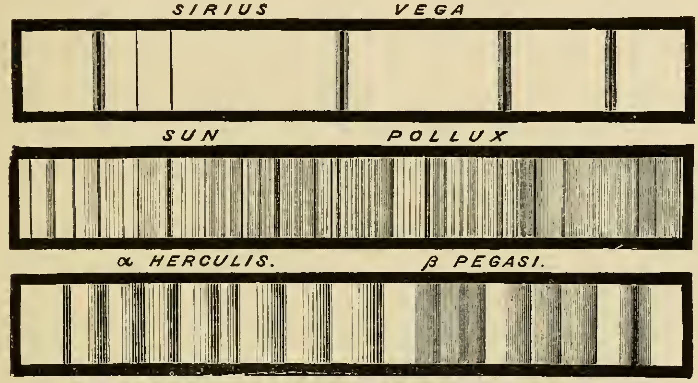

FIG. 74.—Three chief types of stellar spectra.

From one end of the spectrum to the other there are not above half-a-dozen prominent lines. In the next part however we have a star of Type II, which is remarkably like our own sun, both as regards the number of lines and their arrangement. In the lower part of the diagram we have, on the other hand, a star in which we get flutings instead of lines; so that we get not only a difference of degree, but a fundamental spectroscopic difference of kind.

Now there is a circumstance connected with stars of the first type—those with the simple spectrum—very striking to any one in the habit of observing the sun, and it is this: those lines visible in the star, which, be it remembered, has been independently determined to be hotter than our sun, are precisely those lines, and none other, which we often see bright on the disk of the sun itself. I have emphasised the fact that we have independent evidence that the star with very few lines is hotter than our sun. It is also presumable that the stars with the fluted spectra are much cooler than our sun, because they are red stars the light of which is exceedingly feeble; on grounds independent altogether of spectroscopic evidence, they are supposed to be stars in the last visible stage of cooling.

3. *The Question of Multiple Spectra.*

The fact that some elementary bodies have double spectra, that is, that under changed conditions of temperature or electric tension they gave us now a fluted spectrum and now one composed of lines, was independently discovered by Plücker and the younger Mitscherlich.[1] Mitscherlich, in the clearest manner, at once pointed out that this fact might be taken as evidence that the elements on which he experimented were in reality compound bodies.

Plücker, afterwards joined by Hittorf, called the different spectra seen under different conditions spectra of the first and second orders. On this point they wrote seventeen years ago:—[2]

"The first fact which we discovered in operating with our tubes . . . was the following one:—

"*There is a certain number of elementary substances which, when differently heated, furnish two kinds of spectra of quite a different character, not having any line or band in common.*

"The fact is important, as well with regard to theoretical conceptions as to practical applications—the more so as the passage from one kind of spectrum to the other is by no means a continuous one, but takes place abruptly. By regulating the temperature you may repeat the two spectra in any succession *ad libitum.*"

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[1] See *Phil. Mag.* 1864, vol. xxvii. p. 46, and vol. xxviii. p. 187—8.

[2] Plücker and Hittorf on "The Spectra of Ignited Gases and Vapours," *Phil. Trans.* Royal Society, 1865, part i. p. 6.

Ångström, whose name must ever be mentioned with the highest respect by any worker in spectrum analysis, was distinctly opposed to this view, and in the text which accompanies his *Spectre Normal*, he states his opinion in a very distinct manner.[1]

Ångström did not object merely on theoretical grounds. He saw, or thought he saw, room to ascribe all these fluted spectra to impurities.

He was strengthened in this view by observing how, in the case of the spectra of known compounds, there were always flutings in one part of the spectrum or another; a rapid induction naturally, therefore, ascribed all flutings to compounds. The continuity of the gaseous and liquid states of matter, let alone the continuity of Nature's processes generally, never entered into the question. For Ångström, as for many modern chemists, there was no such thing as evolution, no possibility of a close physical relationship between elements, so-called, driven to incandescence from the solid state, and binary compounds of those elements.

In a memoir, however, which appeared after Ångström's death, and which, though under a different title, was in all probability the one referred to, this opinion was to a large extent recalled, and in favour of Plücker's and Mitscherlich's view.[2]

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[1] "Dans un Mémoire sur *les spectres 'doubles'* des corps élémentaires que nous publierons prochainement, M. Thalén et moi, dans les Actes de la Société des Sciences d'Upsal, nous traiterons d'une manière suffisamment complète les questions importantes qu'on peut se proposer sur cet intéressant sujet. Pour le présent, je me borne à dire que les résultats auxquels nous sommes arrivés, ne confirment aucunement l'opinion émise par Plücker, qu'un corps élémentaire pourrait donner, suivant sa température plus ou moins elévée, des spectres tout-à-fait différents. C'est le contraire qui est exact. En effet en augmentant successivement la température, on trouve que les raies varient en intensité d'une manière très-compliquée, et que, par suite, de nouvelles raies peuvent même se présenter, si la tempèrature s'élève suffisamment. Mais, independamment de toutes ces mutations, le spectre d'un certain corps conservera toujours son caractère individuel."—Ångström sur *Le Spectre normal du Soleil*, pages 38, 39.

[2] This latest outcome of Ångström's is so important that I quote his words :— ". . . Nous ne nions certainement pas qu'un corps simple ne puisse dans certains

I say that Ångström recalled his own in favour of Plücker and Mitscherlich's view, because (as it has been remarked by Dr. Schuster[1]) the word "element" is used in a special sense—because in reality allotropic states are classed as compounds, that particular allotropic state which is to be regarded as truly elemental not being stated, nor any reason given why one should thus be singled out.

In the letter to which I have just referred, Dr. Schuster gives an instance in which, in order to show that elementary bodies did not really possess two spectra, a double spectrum was assigned to an acknowledged compound; the fluted spectra of hydrogen and carbon which differ from each other as widely as fluted spectra can, being both ascribed to an impurity of acetylene.

So that those observers who saw in these phenomena nothing but impurities, were perfectly content to give an explanation

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cas donner différents spectres. Citons, par exemple, le spectre d'absorption d'iode que ne ressemble en aucune façon au système des raies brillantes du même corps, obtenues au moyen de l'électricité ; et remarquons de plus qu'en général tout corps simple, présentant la propriété d'allotropie, doit donner à l'état d'incandescence des spectres différents, pourvu que la dite propriété de la substance subsiste non seulement à l'état gazeux du corps, mais encore à la température même de l'incandescence. . . .

"Le soufre solid possède, somme on sait, plusieurs états allotropiques, et, d'après certaines observations, ce corps, même à son état gazeux, prendrait des formes différentes. Par consequent, en supposant que cela soit vrai, le soufre gazeux doit donner plusieurs spectres d'absorption, tandis que la possibilité d'un seul ou de plusieurs spectres brillants dépendra de la circonstance suivante, savoir, si les états allotropiques plus complexes de cette substance supporteront la température de l'incandescence avant de se décomposer.

"Il est bien évident que les cas dont nous venons de parler ne forment pas une exception à la loi générale énoncée ci-dessus, savoir, que chaque corps simple ne peut donner qu'un seul spectre. En effet, si l'on suppose que l'état allotropique est dû à la constitution moléculaire du corps, soit que les molécules se combinent les unes avec les autres, soit qu'elles s'arrangent entre elles d'une certaine manière, cet état allotropique possèdera, au point de vue spectroscopique, toutes les propriétés significatives d'un corps composé, et par conséquent il doit être décomposé de la même façon que celui-ci par les effets de la décharge disruptive de l'électricité."—Ångström and Thalén's *Recherches sur les Spectres des Métalloides*, p. 5.

[1] *Nature*, vol. xv. p. 447.

which would be quite right, provided hydrogen and carbon could only be supposed to have one spectrum; the *impurity* acetylene having two.

In 1868 Wüllner, who had followed Plücker's work from 1863, strongly supported his view of the existence of double spectra, indicating at the same time that the difference of temperature must be regarded as the sole cause of the phenomenon, adding, however, a "decomposition into further elements is not to be thought of." In the case of hydrogen he showed that the banded spectrum ascribed to acetylene really depended upon a change in the emissive power brought about by an alteration of temperature. Touching oxygen, he showed that three distinct spectra may be obtained, while in nitrogen two are observed.

I may say that in my early laboratory experiments I was at first led to think that, in the case of metallic vapours, Ångtröm's first expressed opinion was correct, and I said so. But after more experience and knowledge had been acquired I was compelled by the stern logic of facts to abandon it.

Salet in his admirable work on the Spectra of the Metalloids, was driven to the conclusion that many of these bodies must be held to possess two spectra.[1]

Although, however, in the views I have expressed I have, as it will be seen, simply followed in the footsteps of such men as Mitscherlich, Plücker, and Ångström, and later of Salet and Dr. Schuster, not to mention others, I am aware that though there is a general consensus among spectroscopic workers that double spectra cannot be ascribed to impurities, that consensus is not perhaps even yet quite absolute.

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[1] His conclusions are thus expressed :—

"Nous avons comparé le spectre d'absorption du brome et de l'iode à leur spectre électrique, et cette comparaison nous semble mettre hors de doute la possibilité des spectres doubles. . . .

"Nous avons obtenu, par voie électrique, un spectre primaire de l'iode correspondant à son spectre d'absorption.

"Le soufre, le sélénium et le tellure nous ont offert des spectres de combustion très-analogues aux spectres primaires obtenus par voie électrique, mais différent essentiellement des spectres de lignes. . . .

"Nous avons produit le spectre primaire de l'azote avec différents corps qui n'ont absolument de commun que l'azote ; nous pensons donc avoir démontré qu'il appartient bien réellement a ce métalloïde."—*Annales de Chemie et de Physique*, 4 série, tome xxviii. pp. 70, 71, 1873.

4. *The Variations of Spectral Lines.*

We now approach details of another order—details which have a wide range. I refer to the different intensities of lines observed in terrestrial light-sources under the different conditions in which the phenomena occur, and in different celestial light-sources. It is not my fault that for purposes of definition I have to use language inconsistent with the received views of spectrum analysis twenty years ago, when spectra were supposed to be as changeless as the laws of the Medes and Persians.

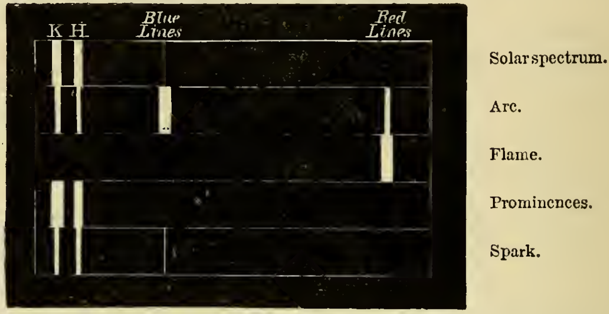

FIG. 75.—The varying intensities of the lines of Calcium as seen under different conditions.

Let us first consider the facts in the case of calcium.

The accompanying diagram will give an idea of the facts observed and of the enormous variations exhibited. We can readily differentiate the lines, and further see how great the changes are when we pass from the laboratory either to the general spectrum of the sun or to the spectrum of any part of it. Here we have the reason why I have chosen to start with calcium; for H and K, the two most marked lines in the solar spectrum, are really coincident with calcium lines.

But with which? With precisely those not seen at all in some cases. And how then about the very strong line of calcium seen in the electric arc, a line excelling all others in brilliancy? In the solar spectrum it is a poor, feeble line.

Let us next pass to the prominences. I have already stated that Young's relative numbers for H, K, and the blue line, are 75, 50, and 3! Even this is not the only differentiation possible. Young, in his work at Sherman, saw the H and K lines brightened over every important spot, and he never saw the blue line brightened at all.

In the case of calcium, at all events, then, both on the terrestrial and the solar evidence, whatever the explanation may be, there is the undoubted fact that fundamental changes of intensity in the lines are to be noticed, and if Kirchhoff's statement about the matching of the intensities of lines in solar and terrestrial spectra is true for one condition, it is of necessity false for all the others.

To sum up the facts regarding calcium, we have first of all the H and K lines differentiated from the others by their greater frequency in prominences, their brightening in spots, and their enormous thickness in the general spectrum of the sun. We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence from solar storms in which they are almost universally seen.

Last stage of all, we have the red lines seen of surpassing brilliancy in the Bunsen flame, while they never appear in solar phenomena.

We next turn to magnesium. The accompanying woodcut gives a generalized view of the phenomena recorded in our laboratories and observatories.

It is easily seen how each line almost can be differentiated from all the rest.

The lines in the yellow-green, the green, and two lines in the blue, marked respectively in the diagram as *a*, *b*, *c*, and *e*, are differentiated from the third line in the blue, *d*, by being seen in the solar spectrum while *d* is not. The line in the yellow-green and the least refrangible of the three blue lines are distinguished from the *b* lines, and the most refrangible of the blue lines by being invisible in the prominences.

The middle line of the three blue ones (*d*) is differentiated from the line in the yellow (*a*) by being seen in the Bunsen flame while the yellow line is invisible.

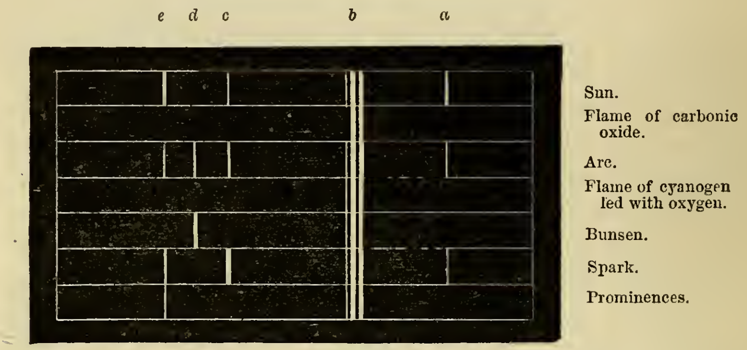

FIG. 76.—Showing the various intensities of the lines of Magnesium as seen under different conditions.

The *b* lines are separated from the others by being seen alone when magnesium ribbon is burnt in a cyanogen flame fed with oxygen, and the two less refrangible members of the *b* group are differentiated from the third line by being seen alone when magnesium ribbon is burnt in the flame of carbonic oxide.

And, finally, the line in the yellow-green (*a*) is distinguished from the least refrangible of the three blue lines (*c*) by being seen strongly reversed in the sun, while the line in the blue is only faintly reversed.

We will now pass to lithium.

Before the maps comparing the long and short lines of some of the chemical elements with the solar spectrum[1] were communicated to the Royal Society, I very carefully tested the work of prior observers on the non-coincidence of the red and orange lines of lithium with the Fraunhofer lines, and found that neither of them was strongly, if at all, represented in the sun. This remark also applies to a line in the blue at wave-length 4603. The lithium line in the violet, however, has a strong representative among the Fraunhofer lines.

Applying, therefore, the previous method of stating the facts, the presence of this line in the sun differentiates it from all the others.

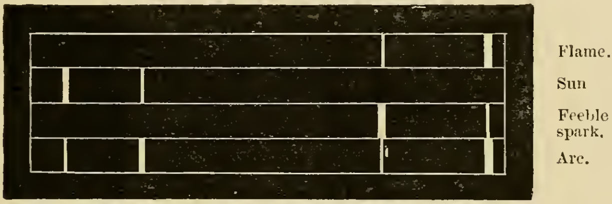

FIG 77.—Showing the various intensities of the lines of Lithium under different conditions.

For the differentiation of the red and yellow lines I need only refer to Bunsen's spectral analytical researches, which were translated in the *Philosophical Magazine*, December, 1875. In plate 4 two spectra of the chloride of lithium are given, one of them showing the red line strong and the yellow one feeble, the other showing merely a trace of the red line, while the intensity of the yellow one is much increased, and a line in the blue is indicated.

Certain observations of Professors Tyndall and Frankland, to be referred to in the sequel, differentiate this blue line from those which are observed under other conditions. The line in the violet to which I have already referred is again differentiated from all the rest by the fact that it is the only line in the spectrum of the sun which is strongly reversed, so far as our present knowledge extends. The various intensities of the lines of lithium, therefore, may be shown as in Fig. 77.

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[1] *Philosophical Transactions*, 1873, Plate 9.

These cases—they might easily be multiplied—are, I think, enough for my present purpose, which is to show that where the early statements favoured the view that the line spectrum of a body was always the same, and that the solar spectrum exactly matched it, further work brought out great differences in the intensities of the different lines observed,—differences amounting to the complete absence of some of the lines under some conditions. The matching then between terrestrial and solar lines at once fell to the ground.

CHAPTER XV.

A POSSIBLE WAY OUT OF THEM.

We can easily understand, seeing that much of the spectroscopic work which had been done up to 1874 had had for its object the connecting—intermingling, so to speak—of solar, stellar, and terrestrial chemistry, that it was not a pleasant thing to find that the path seemed about to be such a very rugged one—that we seemed after all not to be in the light, but in the dark; and because it was not probable that an observer, impressed with the difficulties referred to in the preceding chapters, would long be content with the position, the very practical question was, what was to be done? It became necessary to see if there was any way out of the difficulties. But how was one to attempt to grapple with them? Was it the time to found new theories? or to rest and be thankful? Was it not better to appeal to what was known; to proceed in accordance with Newton's laws of philosophising; and start no new principle unless one was absolutely bound to do so: to appeal, in fact, to the law of continuity?

How would the law of continuity help us? In this way: All unconsciously, spectroscopists had been working under more transcendental conditions as regards temperature than had ever been employed before. Now, seeing that the history of chemistry had been the history of simplification by heat, was it not probable that this higher temperature had done for the matter on which they had experimented exactly what all lower temperatures had been found to do? that is to say, broken the matter up. In other words, when we subjected, say iron, to one of these transcendental temperatures, we were possibly no longer dealing with the spectrum of iron itself, but with the spectrum of the constituents of iron revealed to us by a temperature at which no chemical experiments had been made before.

Indeed, if we appealed to facts, there were facts which appeared to strengthen this view. The spectroscopic behaviour of bodies known to be compound was known to us, and at once suggested an explanation of many of the phenomena which had been observed. Hence, then, there did seem to be a way out of the difficulties which it was worth while to consider.

In a paper communicated to the Royal Society in 1873, dealing, among other things, with the discovery of some new metals in the solar atmosphere, I wrote as follows:—[1]

> I. The absorption of some elementary and compound gases is limited to the most refrangible part of the spectrum when the gases are rare, and creeps gradually into the visible violet part and finally to the red end of the spectrum as the pressure is increased.
>
> II. Both the general and selective absorption of the photospheric light are greater (and therefore the temperature of the photosphere of the sun is higher) than has been supposed.
>
> III. The lines of compounds of a metal and iodine, bromine, &c. are observed generally in the red end of the spectrum, and this holds good for absorption in the case of aqueous vapour.
>
> Such spectra, like those of the metalloids are separated spectroscopically from those of the metallic elements by their columnar or banded structure.
>
> IV. There are, in all probability, no compounds ordinarily present in the sun's reversing layer.

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[1] Bakerian Lecture, 1873; *Phil. Trans.* vol. clxiv. part 2, p. 491.

V. When a metallic compound vapour, such as is referred to in III., is dissociated by the spark, the band spectrum dies out, and the elemental lines come in, according to the degree of temperature employed.

Again, although our knowledge of the spectra of stars is lamentably incomplete, I gather the following facts from the work already accomplished with marvellous skill and industry by Secchi of Rome :—

VI. The sun, so far as its spectrum goes, may be regarded as a representative of class (β) intermediate between stars (α) with much simpler spectra of the same kind, and stars (γ) with much more complex spectra of a different kind.

VII. Sirius, as a type of α, is (1) the brightest (and therefore hottest ?) star in our northern sky ; (2) the blue end of its spectrum is open ; it is only certainly known to contain hydrogen, the other metallic lines being exceedingly thin, thus indicating a small proportion of metallic vapour ; while (3) *the hydrogen lines in this star are enormously distended*, showing that the chromosphere is largely composed of that element.

There are other bright stars of this class.

VIII. As types of class (γ) the red stars may be quoted, the spectra of which are composed of channelled spaces and bands, and in which naturally the blue end is closed. Hence the reversing layers of these stars probably contain metalloids, or compounds, or both, in great quantity ; and in their spectra not only is hydrogen absent, but the metallic lines are reduced in thickness and intensity, which in the light of V., *ante*, may indicate that the metallic vapours are being *associated*. It is fair to assume that these stars are of a lower temperature than our sun.

I have asked myself whether all the above facts cannot be grouped together in a working hypothesis which assumes that in the reversing layers of the sun and stars various degrees of "celestial dissociation" are at work, which dissociation prevents the coming together of the atoms which, at the temperature of the earth and at all artificial temperatures yet attained here, compose the metals, the metalloids, and compounds.

On this working hypothesis, the so-called elements not present in the reversing layer of a star will be in course of formation in the coronal atmosphere and in course of destruction as their vapour-densities carry them down; and their absorption will not only be small in consequence of the reduced pressure of that region, but what absorption there is will probably be limited wholly or in great part to the invisible violet end of the spectrum in the case of such bodies as the pure gases and their combinations and chlorine (see I. *antè*).

The spectroscopic evidence as to what may be called the plasticity of the molecules of the metalloids, including of course oxygen and nitrogen, *but excluding hydrogen*, is so overwhelming, that even the absorption of iodine, although generally it is transparent to violet light, may (as I have found in a repetition of Dr. Andrews's experiments on the dichroism of iodine, in which I observed the spectrum) in part be driven into the violet end of the spectrum, for iodine in a solution in water or alcohol at once gives up its ordinary absorption properties and stops violet light.[1]

Should subsequent researches strengthen the probability of this working hypothesis, it seems possible that iron meteorites will be associated with the metallic stars and stony meteorites with metalloidal and compound stars. Of the iron group of metals in the sun, iron and nickel are those which exist in greatest quantity, as I have determined from the number of lines reversed. Other striking facts, such as the presence of hydrogen in meteorites, might also be referred to.

An interesting physical speculation connected with this working hypothesis is the effect on the period of duration of a star's heat which would be brought about by assuming that the original atoms of which a star is composed are possessed with the increased potential energy of combination which this hypothesis endows them with. From the earliest phase of a star's life the dissipation of energy would, as it were, bring into play a new supply of heat, and so prolong the star's light.

After this paper was communicated to the Royal Society I sent a communication to the Paris Academy on the same subject, and in a private letter to M. Dumas, the perpetual secretary, expanded the reference to the evidence afforded by the stars, as follows :—

---

[1] I have since obtained the same result by observing the absorption of iodine vapour in a white-hot tube.

> "Il semble que plus une étoile est chaude plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques.[1]
>
> "Ainsi nous avons :
>
> "1°. Des étoiles très brillantes où nous ne voyons que l'hydrogène *en quantité énorme* et le magnésium ;
>
> "2°. Des étoiles plus froides, comme notre soleil, où nous trouvons :
>
> $$H. + Mg + Na.$$
> $$H + Mg + Na + Ca.Fe, \&c.,$$
>
> dans ces étoiles, pas de métalloïdes ;
>
> "3°. Des étoiles plus froides encore, dans lesquelles *tous les éléments métalliques sont associés*, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des metalloïdes et des composés.
>
> "4°. Plus une étoile est âgée, plus l'hydrogène libre disparaît ; sur la terre, nous ne trouvons plus l'hydrogène en liberté.
>
> "Il me semble que ces faits sont les preuves de plusieurs idées émises par vous. J'ai pensé que nous pouvions imaginer une '*dissociation céleste*' que continue le travail de nos fourneaux, et que les métalloïdes sont des composés qui sont dissociés par la température solaire, pendant que les éléments métalliques monoatomiques, dont les poids atomiques sont les moindres, sont précisement ceux qui résistent, même à la température des étoiles les plus chaudes."

Before proceeding further, I may perhaps be permitted to anticipate later work somewhat by stating that while observations of the sun have since shown that calcium should be introduced between hydrogen and magnesium for that luminary, Dr. Huggins's earliest photographs demonstrated the same fact for the stars, so that, the chain became much more complete and independent of all hypothesis, the facts could be represented as follows:—[1]

---

[1] The old atomic weights were here in question.

Hottest Stars	Lines of	H + Ca + Mg.
Sun		H + Ca + Mg + Na + Fe.
Cooler Stars		— + Ca + Mg + Na + Fe + Bi + Hg.
Coolest		— — — — Fluted Spectra of Metals and Metalloids.

With every increase of our knowledge the conclusion would still stare us in the face that *the running down of temperature in a mass of matter, which is eventually to form a cool body like the earth, is accompanied by a gradually increasing complexity of chemical forms,* that is, if we are justified in arguing for increased complexity of chemical forms from increased complexity of spectrum.

One of my objects in referring to the communication to the Academy of Science is to enable me to glance at the interesting discussion which followed, in which MM. Dumas and Berthelot took part.

M. Berthelot summed up his remarks as follows:—

> "Entre les corps composés que nous connaissons et leurs polymères, il existe donc cette relation générale, que la chaleur spécifique atomique d'un polymère est à peu près un multiple de celle du corps non condensé.
>
> "Au contraire, la chaleur spécifique atomique demeure constante pour les divers éléments dont les poids atomiques sont multiples les uns des autres. Les mêmes difficultés existent pour l'hypothèse d'un corps simple dont le poids atomique serait la somme des poids atomiques de deux autres.
>
> "Il y a donc entre les propriétés physiques des éléments et celles de leurs composés une opposition singulière et qui donne à réfléchir ; elle est d'autant plus importante que la notion de chaleur spécifique est une traduction du travail moléculaire général, par lequel tous les corps sont maintenus en équilibre de température les uns avec les autres. Cette opposition ne prouve nullement, et je ne voudrais

---

[1] Symbols are used here to save space. H = Hydrogen, Ca = Calcium, Mg = Magnesium, Na = Sodium, Fe = Iron, Bi = Bismuth, Hg = Mercury.

pas que l'on se méprît sur ma pensée à cet égard, l'impossibilité théorique de décomposer nos éléments actuels; mais elle définit mieux les conditions du problème et elle conduit à penser que la décomposition de nos corps simples, si elle pouvait avoir lieu, devrait être accompagnée par des phénomènes d'un tout autre ordre que ceux qui déterminent jusqu'ici la destruction de nos corps composés."

In reply M. Dumas pointed out that M. Berthelot had limited the discussion to heat vibrations, and showed that others might really be in question.

"Les remarques de M. Berthelot, sont parfaitement correctes, en tant qu'elles s'appliquent au mode de vibration de l'éther que nous appelons *chaleur.* Elles ne s'appliqueraient plus à tout autre mode de vibration, à celui qui est nécessaire peut-être pour décomposer un corps répute simple. Comme il veut bien rappeler le rapprochement que j'avais fait, autrefois, entre les radicaux organiques et les éléments minéraux, il me permettra d'ajouter que les *différences* qu'il signale entre eux m'étaient bien connues (*Leçons de Philosophie Chimique,* 1836, p. 280) et qu'elles ne m'avaient pas semblé suffisantes pour combattre les conclusions dérivées des *analogies* saisissantes que j'avais signalées un peu plus tard.

"Mais M. Berthelot accorde, en terminant, tout ce que sont disposées à admettre les personnes qui pensent que ce qui doit prédominer, dans ces questions, c'est le sentiment de la *continuité* dans les caractères des êtres et dans les phénomènes de la nature.

. . . . . .

En résumé, quand je soutenais devant l'Académie que les éléments de Lavoisier devaient être considérés, ainsi qu'il avait établi lui-même, non comme les éléments *absolus* de l'univers, mais comme les éléments *relatifs* de l'experience humaine; quand je professais, il y a longtemps, que l'*hydrogène* était plus près des *métaux* que de toute autre classe de corps; j'émettais des opinions que les découvertes actuelles viennent confirmer et que je n'ai point à modifier aujourd'hui."

Having thus referred to the opinion to which my work had led me in 1873, I must now state that although I did not know it till long afterwards, I had been completely anticipated as touching the main point of it by two eminent men—one of them, alas! no longer with us—I refer to the late Sir Benjamin Brodie—and Professor Sterry Hunt.

Sir B. Brodie in Part I. of his *Calculus of Chemical Operations,* read before the Royal Society on May 3, 1866,[1] was led by his mathematical treatment of chemical phenomena to assume the existence of certain ideal elements. These, he said, "though now revealed to us through the numerical properties of chemical equations only as *implicit and dependent existences,* we cannot but surmise may sometimes become, or may in the past have been, *isolated and independent existences.*"

The influence of this view on Professor Sterry Hunt had best be given in his own words:—[2]

> "Shortly after this publication, in the spring of 1867, I spent several days in Paris with the late Henri Sainte-Claire Deville, repeating with him some of his remarkable experiments in chemical dissociation, the theory of which we then discussed in its relations to Faye's solar hypothesis. From Paris, in the month of May, I went, as the guest of Brodie, for a few days to Oxford, where I read for the first time and discussed with him his essay on the *Calculus of Chemical Operations* in which connection occurred the very natural suggestion that his ideal elements might perhaps be liberated in solar fires, and thus be made evident to the spectroscope. I was then about to give, by invitation, a lecture before the Royal Institution on "The Chemistry of the Primeval Earth, which was delivered May 31, 1867. A stenographic report of the lecture, revised by the author, was published in the *Chemical News* of June 21, 1867, and in the *Proceedings of the Royal Institution.* Therein I considered the chemistry of nebulæ, sun, and stars in the combined light of spectroscopic analysis and Deville's researches on dissociation, and concluded with the generalisation that the 'breaking-up of compounds, or dissociation of elements, by intense heat,' is a principle of universal application, so that we may suppose that all the elements which make up the sun, or our planet, would, when so intensely heated as to be in the gaseous condition which all matter is capable of assuming, remain uncombined,—that is to say, would exist together in the state of chemical elements,—

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[1] *Phil. Trans.* 1866.

[2] *Chem. News,* 1882, vol. xlv. p. 74.

whose further dissociation in stellar or nebulous masses may even give us evidence of matter still more elemental than that revealed in the experiments of the laboratory, where we can only conjecture the compound nature of many of the so-called elementary substances."

On the 6th of June, one week after Dr. Sterry Hunt's lecture, Sir Benjamin Brodie gave a lecture on "Ideal Chemistry" before the Chemical Society of London,[1] in which he further extended the suggestion already put forth by him in his memoir of 1866 :—

> "We may conceive that in remote ages the temperature of matter was much higher than it is now, and that these other things [the ideal elements] existed in the state of perfect gases—separate existences—uncombined."

He further suggested, from spectroscopic evidence, that it is probable that "we may one day, from this source, have revealed to us independent evidence of the existence of these ideal elements in the sun and stars."

Thus we see that Brodie[2] by a stroke of genius, before anything was known about the chemistry of the sun, went to the sun for that transcendental temperature he was in search of; thus showing that he had an absolutely pure and accurate conception of the whole thing as I believe it to be—but that is to anticipate matters. He suggested that the constituents of our elementary bodies might be found existing as independent forms in the hottest parts of the solar atmosphere.

Would it have been wise to have considered, *then*, the whole question of the dissociation of elementary bodies? I think it would not have been wise; data there were, as I have endeavoured to show, but they were insufficient. The true thing to be done was to endeavour to accumulate new facts and then to see what would happen when a sufficiently long base had been obtained.

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[1] *Chem. News*, 1867, June 14.

[2] *Ideal Chemistry.* Lecture delivered to the Chemical Society in 1867, republished 1880. (Macmillan.)

What did we want? We wanted to settle those questions of the variations of spectra seen in our laboratories, and the variations observed when we passed from the spectrum, say of iron, on the earth, to the spectrum of iron in solar spots and storms.

The coincidence with Fraunhofer lines, of lines of different substances seen in our laboratories which had been referred to by Ångström and Kirchhoff, also required investigation. What more ready means of doing this—what more perfect means were there than those placed at our disposal by photography? Photography has no personal equation, it has no inducement to twist a result either in one direction or the other, and it moreover has this excellent thing about it, that the results can be multiplied a thousandfold and can be recorded in an absolutely easy and safe manner. There were other reasons why photography should be introduced in this part of the work. We see at once that it was quite easy to introduce the process of purification of the spectra to which I have already drawn attention, by merely comparing a series of photographs. Again, it was quite possible by the use of the electric lamp to very considerably surpass the dispersion which Ångström had employed. So that, if impurities had been suggested, there was now a method which has not yet been challenged of getting rid of them; if the dispersion was then insufficient there was nothing to prevent it being made very much more considerable, because a perfect photograph will bear a very considerable amount of magnification.

There were also many other obvious lines of research, especially some in connection with multiple spectra, which seemed to promise new facts. But the upshot of the whole matter was that more work was wanted, and that in this work photography should be utilized.

CHAPTER XVI.

INTRODUCTION OF PHOTOGRAPHY.

1. *Method Employed.*

THE method of photography that was adopted may now be stated. One object was to compare the light of the sun with the light of the vapour, in the electric arc, of any particular substance that we wished to observe. Such a comparison of the Fraunhofer lines, with those visible in the spectrum of the vapour of each of the metallic elements would enable us to study each line in detail and finally to construct a new map of the solar spectrum.

But it may be said, "Surely if you are going to limit yourselt to photography, you will only be dealing with a very small part of the spectrum." My reply to that is that the remark is perfectly true, but practically the portion thus investigated was a new one, and so it gave us fresh ground on which to base our inquiries. But it was not long before other laboratory work gave us reason to believe that what was then being done in photography at the blue end of the spectrum would be done by photography in every other portion, for in fact a spectroscopic study of the behaviour of bodies at low temperature, to which I shall refer in the sequel, had led several to believe—at all events had led me to believe—that what one got in the text-books about actinism and so on was but a very rough approximation to the

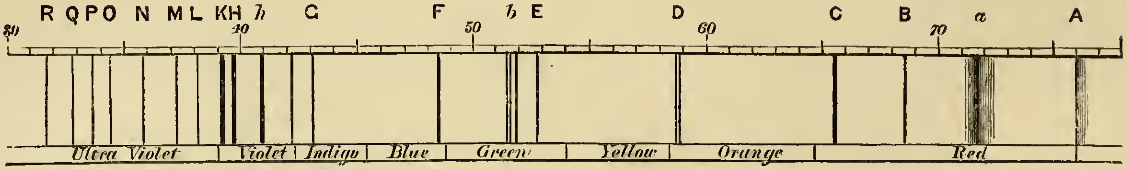

FIG 78.—Wave-length map of the solar spectrum. The visible portion extends from A to K. The portion that could be photographed in 1874 extends from *b* to R.

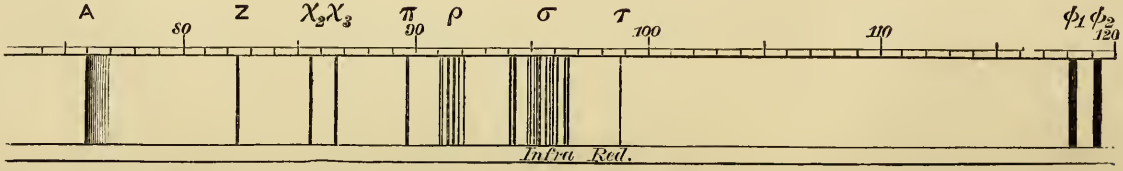

FIG. 79.—Map of the infra-red portion of the solar spectrum photographed by Captain Abney in addition to that shown in Fig. 78.

truth. We had been taking as the functions of light what were really the functions of the bodies which received it, and it was, therefore, quite easy to imagine, and one was justified in hoping, that as the work went on we should find that what one particular kind of substance would do for the blue rays, another particular kind of substance would do for the red rays and for the green rays, and so on.

My reference to this at the time was as follows :—

"I cannot but think, moreover, that when the light which the spectroscope has already thrown upon molecular action shall be better known and used as a basis for further inquiry, methods of photography greatly exceeding the present one in rapidity, in the less-refrangible portion of the spectrum, will be developed and utilized in the research."[1] Fig. 79 will show how fully this prophecy has been fulfilled by the results obtained by Captain Abney within the last few years.

In order to carry on this work a spectroscope of large dispersion was necessary, and its observing telescope was replaced by a camera of long focal length. In the instrument used at the commencement of the research the whole solar spectrum from beyond H to the red fell upon a five-inch plate.

The laboratory in which the work was carried on had a window nearly due (magnetic) south. Outside the window level slate slabs were placed as supports for a heliostat. The spectroscope was supported on a platform on rollers, the height of the platform being such that the horizontal beam from the heliostat was coincident with the axis of the collimator. This general arrangement is shown in Fig. 80.

In order to obtain photographs of the *solar spectrum* of the very best kind, it was necessary to limit the beam passing through the prisms to one of very small dimensions—a method previously employed with such admirable results by Mr. Rutherfurd.

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[1] *Proc. Roy. Soc.* No. 158, 1875.

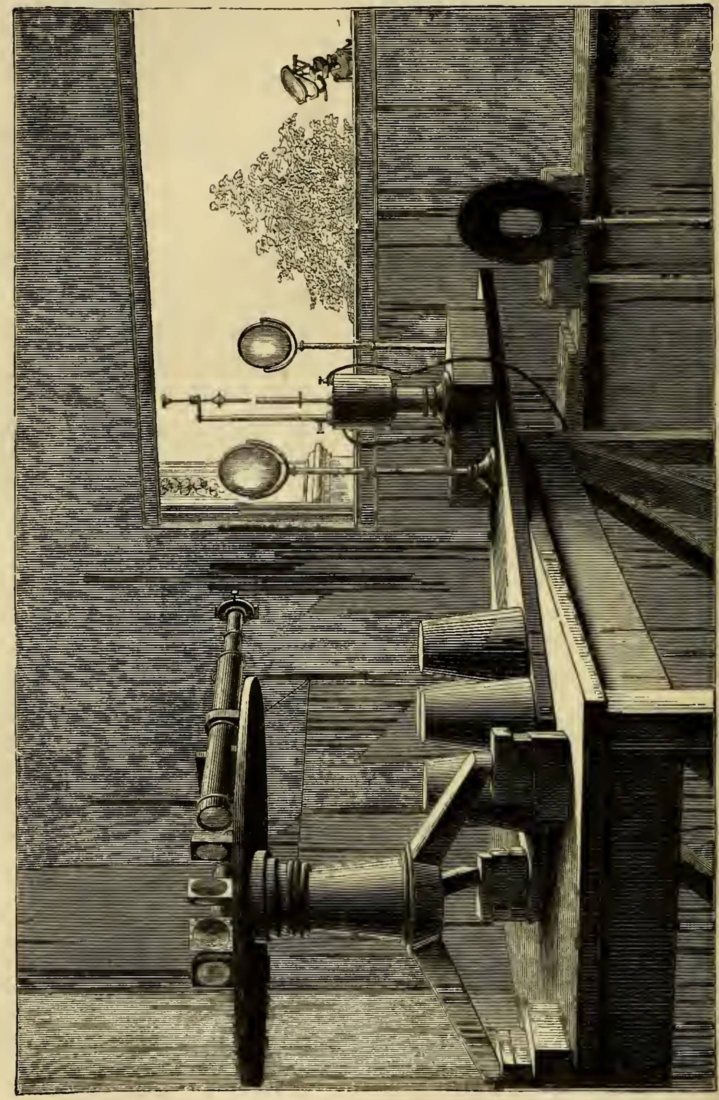

Fig. 80.—Arrangement for photographically determining the coincidences of solar and metallic lines, showing the heliostat, system of lenses, electric lamp, and spectroscope.

In photographing the *long and short lines of metallic spectra*, it was found that this object could not be well attained with the electric lamp in its usual position (with vertical poles), as the central column of dense vapour, as a rule, extended across the arc, *i.e.* from pole to pole, and gave all the short lines. In order to obviate this a horizontal arc was employed. This was accomplished by placing the lamp on its side and firmly securing it in that position. An image of the horizontal arc was then thrown on the vertical slit in the usual manner.

The lines thus photographed are pointed at either end, and disappear from the centre in the order of their length, so that an exquisitely symmetrical double or duplicate determination of their lengths is thus obtained.

Although the lengths, thicknesses, intensities, and reversals of the lines were thus readily recorded, we had so far no very convenient method by which to fix their positions, or to determine their coincidence or non-coincidence with solar lines. In order to accomplish these ends, I resolved to photograph the solar spectrum on each plate immediately above or below the metallic spectrum under examination. To do this an extension of the method of working hitherto in use was introduced, depending upon the following considerations:—

It is obvious that when we observe a spectrum its breadth will depend upon the length of the slit. When we at the same time illuminate different portions of the slit with rays proceeding from different vapours, the spectra of the different light-sources are seen at once. But when we introduce photography we can more conveniently obtain results by illuminating *successively* different portions of the slit, the effect being that the various spectra will successively record themselves on different portions of the photographic plate.

Acting on this principle I first covered up the upper half of the slit, allowing the image of the horizontal arc to fall centrally on the slit, so that in this way there was impressed an image of half the thickness of the horizonal arc. After this was accomplished the half of the slit first used was covered up, that which was previously closed being opened, and through this newly opened portion sunlight was admitted. But to do this

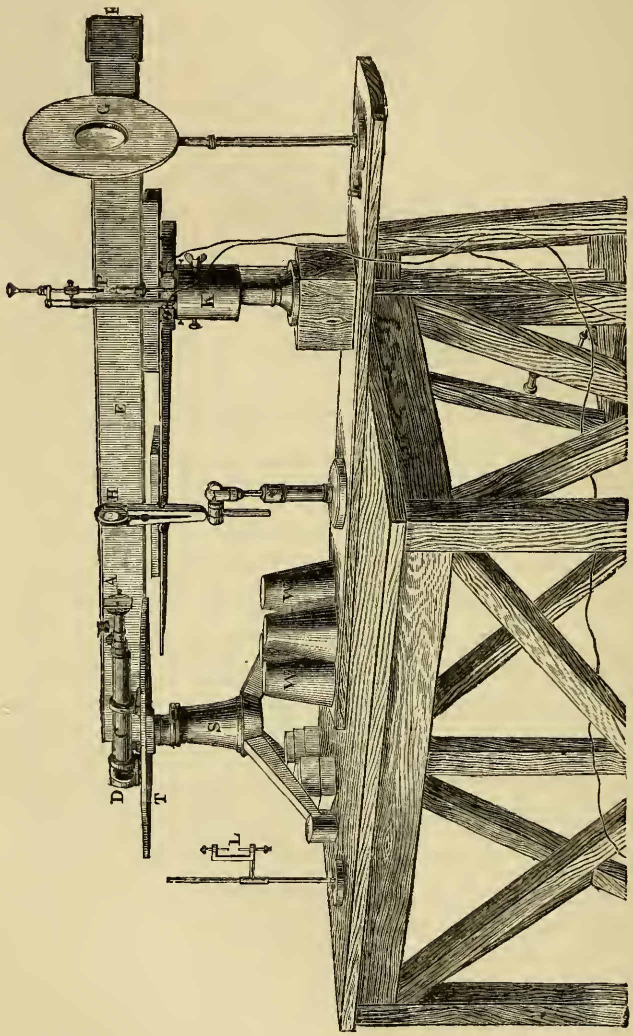

FIG. 81.—Another view of the spectrum photographic arrangements, showing the long camera replacing the telescope employed for eye observations. A, slit; B, focussing screw; D, prisms; E, camera; F, slide-holder; G, lens for focussing image of sun between poles; H, lens for focussing arc in slit; K, electric lamp; L, Spark stand for arc with induction coil; P, poles.

effectively certain precautions were necessary. A description of the method used will show how these were taken.

In addition to the lens placed between the lamp and the slit to throw an image of the arc on the latter, another lens was now introduced between the heliostat and the lamp—heliostat, lenses, lamp, and collimator being of course in the same straight line. The action of the newly interpolated lens was to throw an image of the sun between the poles of the lamp, so that when the spectrum of the arc was properly focussed by the camera lens on to the photographic plate, the solar spectrum, when subsequently thrown in, was also in focus.

So far, then, we had the long and short lines of each substance compared with the solar spectrum on the same plate.

The accompanying diagrams (Figs. 82, 83, and 84) show the arrangements adopted in the cases mentioned.

In that branch of the inquiry which dealt with the causes of the coincidences of the lines in various spectra, it was not essential to refer the lines to the solar spectrum each time; thus if we studied the cases of, say, aluminium and calcium impurities, it was better to photograph the suspected spectra side by side and confront them. To do this all that was necessary was to extend the application of the principle already referred to. In fact the only practical limit to the number of spectra we can get on to one plate is the time the plate takes to dry, and instead of uncovering half of the slit at one time we may uncover any smaller portion.

This was effected by means of a brass shutter with a square opening cut through it, which slides in grooves in front of and up and down the slit. On the outside of the shutter holes were bored, the distance between each hole being the same as the height of the opening in the shutter. A short pin fixed to a spring falls into each hole in succession as the shutter is moved up or down, and so ensures that there shall be neither superposition of spectra nor gaps between them.

By the use of the shutter, four or more spectra showing positions and thicknesses (but not lengths) of the lines could readily be obtained on one plate; and one of these might be the solar spectrum to serve as guide and scale. Subsequently a system of five slides outside the slit was used.

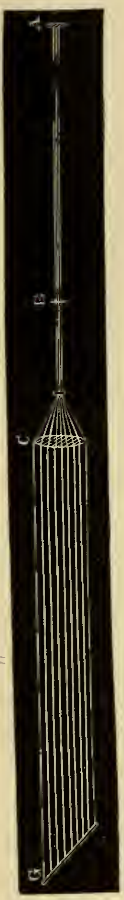

FIG. 82.—Arrangement for obtaining solar spectrum alone.

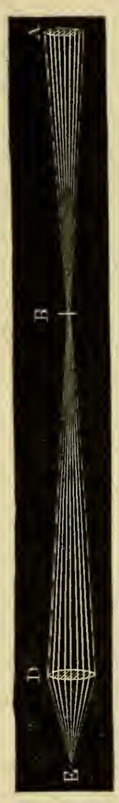

FIG. 83.—Arrangement for obtaining long and short lines.

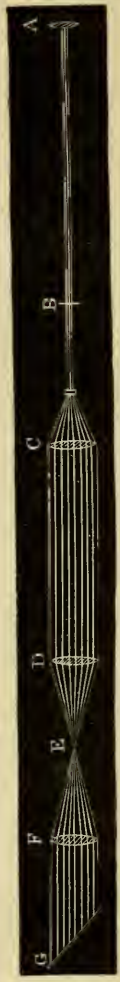

FIG. 84.—Arrangement for obtaining and comparing lines with solar spectrum. A, collimating lens; B, slit; C, opera-glass; G, heliostat; D, lens; E, poles; F, lens throwing image of sun between poles.

In the case of each metallic element, then, we had first, a comparison of its lines in the spectrum of the electric arc photographed with the solar spectrum. After that we had the long and short lines in the same substance photographed on another plate. After that we had all the substances which might exist as impurities in the first substance—that is to say, all the metallic elements photographed with their lines—their long and short lines, in precisely the same manner; and finally we had a comparison of the substance we wished to study, say iron, with a spectrum of every other substance which might exist in it as an impurity. It will be seen, therefore, that an enormous number of photographs had to be taken. The photographs, when obtained, were examined by a powerful lens, and the various lines plotted down on a map.

2. *Some Results.*

I purpose now to deal, as briefly as may be, with some of the results obtained.

It will be clear at once, that as photography enabled us to obtain a direct comparison of the lines visible in the spectrum of each chemical element with the spectrum of the sun, we were now in a position to see if there were any evidence of the existence of substances in the solar atmosphere which had hitherto remained undetected.

Next, as I have already pointed out, we were enabled to trace the lines in all spectra due to mutual impurities, and in this way by careful elimination of such lines to produce maps which should be above suspicion.

The comparison photographs were limited necessarily to the blue and violet portions of the spectrum, because the methods since worked out by Captain Abney for photographing the other regions were not then available, and of set purpose I limited it still more, as I wished to find the *dernier mot* in the present state of science regarding the coincidence of metallic with Fraunhofer lines; and for this it was imperative to work on a large scale over a small region, rather than on a small scale over a large one.

In point of fact, this special work was limited to about the $\frac{1}{100}$th part of the spectrum, and this was mapped on a large scale. A complete map of the spectrum on the scale adopted would be about half a furlong long. The work took time: including interruptions of one kind and another, some four years were expended on it.

Finally, we had a large number of photographs which we could study at our leisure, showing the physical characteristics of the various lines of the different chemical elements and the variations which the lines underwent under different conditions. These were precious records, the many riddles of which are not all, even yet, I think, read. We will take these various matters *seriatim*.

3. *New Metals in the Sun.*

On page 156 I gave an account of the attempt made to determine the existence of additional metals in the sun by using the existing maps and tables in the absence of precise knowledge touching the longest lines. The photographs now came to the aid of the eye observations which had been made on the new method of the spectra of lithium, sodium, magnesium, cobalt, nickel, aluminium, lead, manganese, cadmium, tin, zinc, strontium, antimony, and barium.[1]

In the case of these metals we now had the longest lines from red to violet, and the photographs gave us the longest lines in the violet and blue of the other substances, the less refrangible portions of the spectra of these latter had not been mapped on the new method.

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[1] *Phil. Trans.* 1873, p. 253.

The photographic evidence which was utilized in this portion of the inquiry consisted of the longest lines visible in the respective spectra. For the reasons stated on page 156 the fact of these lines being reversed in the solar spectrum must be considered as strong evidence in favour of the existence in the sun of the metals to which they belong. Where, however, there is only one line, as with lithium, rubidium, and some other substances, the evidence cannot be considered final, and until a larger number of coincidences is determined, the presence of these metals in the sun's atmosphere can only be said to be probable.

It had, moreover, to be borne in mind that, in addition to the long lines which a spectrum may contain in the red, yellow, or orange, long lines may exist in the ultra-violet region, so that the absence of such metals from the sun cannot be absolutely affirmed until a complete survey of this region has been completed.

The first thing to be done was to see if the photographs confirmed the results already announced in 1873, with regard to strontium, lead, cadmium, cerium, and uranium. They did, as the following table will show:—

Strontium.—Reversal of 4 lines at wave-length 4029·6, 4076·77, 4215·00, and 4607·5.

Lead.—Reversal of 3 lines at w.l. 4019·28, 4056·8, and 4061·25.

Cadmium.—Reversal of 2 lines at w.l. 4677·0 and 4799·00.

Cerium.—Reversal of 2 lines at w.l. 3928·7 and 4012·0.

Uranium.—Reversal of 3 lines at w.l. 3931·0, 3943·0, and 3965·8.

The photographic evidence seemed also conclusive in the case of the following substances:—

Potassium.—Reversal of 2 lines at wave-length 4042·75, and 4046·28 (apparently the only potassium lines in this region of the spectrum).

Vanadium.—Reversal of 4 lines at wave-length 4379·0, 4384·0, 4389·0, and 4407·5.

Palladium.—Reversal of 5 lines at w.l. 3893·0, 3958·0, 4787·0, 4817·0, and 4874·0.

Molybdenum.—Reversal of 4 lines at w.l. 3902·0, 4576·0, 4706·0, and 4730·0.

In many cases the result was still left doubtful.

Indium.—One line at wave-length 4101 was coincident with H, a hydrogen line. The reversal of another line at w.l. 4509·0 was doubtful.

Lithium.—One line at w.l. 4603·0 was reversed, but the reversal of the long red line at w.l. 6705 was not detected.

Rubidium.—One long line at w.l. 4202·0 was reversed, but solar lines corresponding to the long red lines at w.l. 6205 and 6296 were not traced.

Cæsium.—Two lines at w.l. 4554·9 and 4592 were possibly reversed.

Bismuth.—One line at w.l. 4722·0 was reversed, but further evidence was considered necessary.

Tin.—One line at w.l. 4524 was apparently reversed, but further evidence was desirable.

Glucinum.—One line at w.l. 3904·77 was apparently reversed, further evidence desirable.

Lanthanum.—Three winged lines at w.l. 3948·20, 3988·0, and 3995·04 were reversed.

Yttrium, or *Erbium.*—Two lines at w.l. 3981·87 and 3949·55 were reversed.

The upshot of this work, then, comparing it with what had gone before, gave us the following metals existing, with more or less probability, in the atmosphere of the sun :—

Hydrogen, sodium, calcium, magnesium, iron, manganese, titanium, chromium, nickel, barium, zinc, cobalt, aluminium, strontium, lead, cadmium, cerium, uranium, potassium, vanadium, palladium, molybdenum, indium, lithium, rubidium, cæsium, bismuth, tin, glucinum, lanthanum, yttrium or erbium.

Thirty-one metallic substances in all. It will be seen that the photographic attack in this direction was of extreme value.

4. *Physical Peculiarities of Lines.*

In the photographs of the arc spectra one of the first things noticed[1] was, as already stated, that not unfrequently a very thick line reversed itself, a circumstance which greatly facilitates its comparison with confronted lines, since a thin dark line then runs down the centre of the thicker bright one. The absorption line does not always occupy the exact centre of the bright band. Examples of this from the spectra of calcium and aluminium were published at the time.

These phenomena enabled us to draw a distinction between those substances which give us winged and easily reversed lines in arc spectra and those which do not. Among the former are sodium, magnesium, calcium, strontium, silver, and some others. There is no difficulty in seeing the reversal of winged lines in the case of all spectra in which they exist, when the image of the arc is thrown on the slit, and such lines lying in the region between K and G were photographed in 1873. But although the various curious phenomena which these reversals present are easily visible, it is very difficult to photograph them in all their stages.

Still with the long arc given by the Siemens' machine, several of the various aspects put on during the process of reversal can be caught. Chief among these phenomena are the various thicknesses of the lines of reversal over the arc and poles, and the appearance of the bright line without reversal in some regions, and the reversal without the bright line in others.

All the phenomena presented by the absorption of the D line to the eye are in the photographs in duplicate. It may be useful, perhaps, to state what phenomena are seen in the case of the D line, when a small image of the arc, carefully focussed for the yellow light, is thrown upon the slit and considerable dispersion is employed.

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[1] *Phil. Trans.* 1874, part 2, p. 805.

If the arc is observed before the introduction of the sodium between the poles, with the poles slightly separated, the continuous spectrum of each pole will be bounded by a sharp line, and in the included region the exquisite flutings of the carbon vapour will be seen together with the lines due to any metallic substances present. The metallic lines will be thickest near one pole, and will overlap its continuous spectrum, while the carbon flutings will overlap the other. The D lines in the arc should occupy the centre of the field of view.

If now a piece of metallic sodium be placed on the lower pole, the whole of the light will be blotted out, if the field of view be small. Gradually the two ends of the spectrum of the arc will begin to appear on either side of the field, the sharp boundary lines to which reference has been made having disappeared, as the poles are no longer incandescent.

The absorption in its retreat to the central region will next take the appearance of a truncated cone, its base resting on that side of the arc formerly occupied by the carbon flutings. The intense blackness gradually changes into a misty veil through which, as it were, the D lines gradually make their appearance as enormous truncated cones with their bases turned in the opposite direction to that occupied by the original absorption.

The more refrangible line is twice as thick as the other, and is often contorted while the other is rigid. Gradually, as the quantity of sodium vapour is reduced, the poles regain their original incandescence, and the one to which the carbon bands attach themselves wili become more vividly incandescent than the other. Then begins a new set of phenomena—the absorption of the light of either pole. Generally on the more incandescent pole the absorption widens for a space, then narrows, and finally puts on a trumpet appearance and is lost.

Very often on the opposite pole the line is seen merely as a bright one, or again the absorption is reduced to its smallest proportions.[1]

The following general statements may be made with regard to these curious phenomena:—

I. We have first a general absorption of the light of the arc over the region to be eventually occupied by the bright line.

II. Next the disappearance of this indefinite absorption and the formation of a truncated absorption of a symmetrical bright and wider line.

III. Next the parallelism of the boundaries of the bright and dark lines in the centre of the arc itself.

IV. Next the various absorption phenomena on the two poles.

V. Finally the extinction of the absorption line in the arc.

Such phenomena afford a striking instance of the irregular absorption and radiation of the molecules of the same element in the same sectional plane of the arc.

Some lines are clean cut in their reversal; others, again, to use the laboratory phrase, are "fluffy" to a degree that must be seen to be appreciated, so much so, that when photographed they appear merely as blurs upon the plate.[2] In some cases the reversal is seen to widen as we approach the cooler external region of the arc, thus showing absorption increasing with reduction of temperature.

When a Siemens' lamp is employed, the absorption phenomena of the flame are also most curious. The lines which reverse themselves most readily in the arc are generally those, the absorption of which is most developed in the flame; thus the manganese triplet in the violet is magnificently reversed in the flame, and the blue calcium line is thus often seen widened, *H and K being not only not absorbed, but entirely invisible.*

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[1] *Proc. Roy. Soc.* March, No. 194, 1879.

[2] *Proc. Roy. Soc.* 194, 1879. The counterparts of these lines are almost invariably absent from the solar spectrum.

Some spectra are full of doublets: sodium and potassium, as ordinarily mapped, may be said indeed to consist exclusively of doublets; others, again, are full of triplets, the wider member being sometimes on the more, sometimes on the less, refrangible side. Doublets and triplets, as a rule, reverse themselves more freely than the irregular lines in the same spectrum—which particular doublet or triplet will reverse depending upon the temperature. The more one studies these photographic spectra in detail, and especially under varying conditions of temperature *which enable us to observe the reversal now of this set of lines, now of that,* the more complex becomes the possible origin of the phenomena thus permanently recorded.

5. *The Elimination of Impurities.*

The purification of the spectrum of each substance was conducted as follows:—The spectrum of the element, as stated on page 215, was first confronted with the spectra of the substances most likely to be present as impurities. This was most conveniently done by photographing the spectra on the same plate one above the other, so that common lines were continuous.

We had, for instance, a great number of photographs of iron, to take a case. The spectrum of iron was then photographed with that of each of the other metallic elements. The question then was: given these photographs bristling with impurities—and if there were no impurities present we should not know that our photograph was a good one, because the continuous lines showed there was no shift—how are we to produce a map of the spectrum of one substance which shall be absolutely purified with regard to the rest?

The diagram (Fig. 85) will show how the process of elimination which was now rendered possible was carried on. We have there three hypothetical spectra, with their long and short lines. We compare A with B, and find that in the photograph which gives us A compared with B we have so many lines of the two substances. Now we say if B exists in A as an impurity, the longest line of B will be there. We look for the longest line of B, and we find it, and we put a minus sign over that line in A to show it is most probably due to an impurity of B. We then ask if there are any more lines of B in A, and we naturally look for the next longest line of B; we find that, and we put a minus sign over that; and then we look for the next longest line and mark that; then we look for the next one; it is not there —then there are no more lines of B in A. In that way, if we knew everything, we should years ago have been able to map a spectrum of a substance A, from which all traces of the spectroscopic effects due to the presence of a substance B, had been eliminated, and we might go on with substance C, and so on; and in that way eliminate the effects produced by C as well as by B from the spectrum of the substance A.

Lines due to impurities, thus traced, were marked for omission from the map and their true sources recorded, while any line that was observed to vary in length and thickness in the various photographs was at once suspected to be an impurity line, and if traced to such was likewise marked for omission.

For instance, the longest line to the left in C appears as a short line in A. It is therefore omitted from A. The three longest lines in B appear in A. They are therefore omitted from A. While the shortest line in B, which also appears in A, is marked as a common line, because the lines of intermediate length are not represented in A.

The retention or rejection of lines coincident in two or more spectra is determined, then, by observing in which spectrum the line is longest and thickest. Where several elements were mapped at once, all their spectra were confronted on the same plate, as this means the presence of one of the substances as an impurity in the others could be at once detected.

Thus the two lines H and K (3933 and 3968), assigned both to iron and calcium by Ångström, were traced to calcium by the following observations:—

a. The lines are well represented in the spectrum of commercial wrought iron, but are absolutely coincident with two thick lines in the spectrum of calcium chloride with which the iron spectrum has been confronted.

b. The lines are represented by mere traces in the spectrum of a specimen of pure iron prepared by the late Dr. Matthiessen, and obligingly placed at my disposal by Dr. Russell. Both poles of the lamp were of iron, the lower pole consisting of an ingot of the metal which had been cast in a lime-mould.

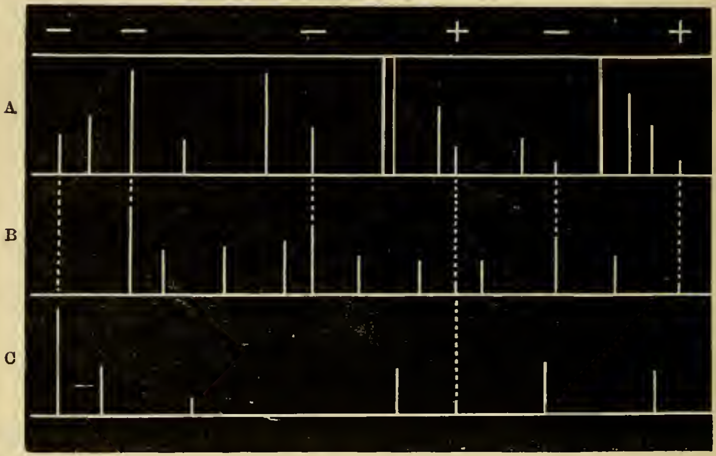

FIG. 85.—Diagram showing the process by which impurities are eliminated from spectra. The lines marked – are due to impurities of one substance in another; those marked + are *not* due to impurities and are therefore common lines.

c. The lines are altogether absent from a photograph of pure iron, where both poles of the lamp were of the pure metal *not* cast in lime, and they are likewise absent in a photograph of the spectrum of the Lenarto meteorite.

These examples serve to illustrate the manner in which large numbers of the coincidences recorded by former observers were disposed of.

By eliminating lines due to impurities in the manner just described, a spectrum is at length obtained, of which every line is assignable to the particular element photographed, *the same temperature being employed in the case of all the elements observed.* This is an important point.

As the mapping progressed, there seemed a probability that the spectra of iron and other metals, which all observers have found very complex, would become much simplified, owing to the elimination of many lines hitherto attributed to these metals being proved in this way to be due to the presence of impurities in them.

It may not be uninteresting to detail a few considerations which induced me to arrive at such a conclusion. Instances occurred in which a well-defined line appearing in the spectrum of a metal has proved coincident with the longest line in the spectrum of an element newly mapped.

Then, again, suppose iron to be present as an impurity in an element which is being mapped for the first time, the longest lines of iron are first looked for, but it may happen that one of these lines is represented in the spectrum of the new element as very much longer than the other lines in the spectrum of iron, which have hitherto been regarded as of about the same length.

Lastly, there were many lines in the spectrum of an element reversed in the solar spectrum, which were coincident with lines of the same wave-length in the spectra of other elements. It might be that greater dispersion would in all these instances prove that these lines instead of being absolutely coincident, might slightly graze one another; but my experience, thus early, led me to suspect that these lines were rather due to the presence of common impurities, either in the shape of unmapped elements or of elements hitherto unknown.

One important side of this work was, that it enabled any spectroscopist or any chemist who chose to take the trouble and devote the time to it, to examine as to the existence of impurities in different substances: not to determine the absolute amount of impurity, but enabling him to say that in specimen A there is a greater impurity of X than there is in specimen B, and so on. The statements were not absolute, they were relative, but if only relative they were certainly a very great advance on anything which had been done before, because until this question of long and short lines was introduced it was impossible to see how to eliminate impurities.

I am the more anxious to insist on this work because it took a very long time to execute, and was of a very rigid nature; and because, so far as I know, no other suggestion has been made with regard to obtaining pure spectra; and of course, if we wish to study the chemistry of the sun, the first desideratum, as Kirchhoff saw, and as Ångström saw, and as we all see now, is to have a series of maps absolutely and completely beyond all suspicion.

CHAPTER XVII.

A STUDY OF THE PURIFIED SPECTRA.

In the year 1878, then, we had, as a result of some years' work, the spectra of the metallic elements purified in the manner indicated in the previous chapter, compared minutely with the spectrum of the sun over a limited region—that near H—and more generally over a much more extended one.

We were now, then, in a position to inquire, in a very definite manner, into a question to which reference has already been frequently made—the coincidence of solar and terrestrial spectra; and further, we could see whether the purification which had been effected gave us in the case of each substance a spectrum peculiar to that substance. Nor was this all. There was reason to hope that the complete working out of the long and short principle might get rid of some of the difficulties referred to in previous chapters; such, for instance, as that connected with the reduction of the number of lines seen in the case of each substance in the hotter regions of the solar atmosphere.

It will be seen that on all these points the more minute inquiry now rendered possible landed us in hopeless confusion, and the difficulties to which attention has already been drawn seemed to become more impossible of explanation the further we went, that is, if we applied to them the ideas then generally received.

1. *Variations between Solar and Terrestrial Spectra.*

Attention has been called to Kirchhoff's statement that the existence of the terrestrial elements in the sun is established by the fact of the coincidence of wave-length and *intensity* between the lines visible in our laboratories and the lines recorded as existing in the solar spectrum.

We have already seen what enormous differences there are in the spectrum of calcium under different conditions. In the diagram of the calcium spectrum (Fig. 75) we saw that H and K, the most important lines in the spectrum of the sun, are really not the thickest lines in the spectrum of that substance at the temperature of the electric arc. But when we pass from calcium, which occupied the attention of observers several years ago, to other elements, as the photographs enable us easily to do, and when we go still more into the minute anatomy of the thing, we find that the further we go the less final is the statement that the matching in intensity of the lines is perfect.

Nor is this all. *Not only is the matching less perfect in intensity, but many lines in various spectra are left out, which omission cannot be accounted for on the long and short principle.* It has been before pointed out that of the 26 lines of aluminium, it is easy to explain how 2 only are left in the solar spectrum, because the 24 dropped were short lines. But when we come to other elements, we find of adjacent lines—lines of equal length, which, so far as we should expect, ought to be equally represented in the sun—one is absent, and one is present, probably with more intensity than it would seem to deserve from its behaviour among other lines of the spectrum. A table will best exhibit the sort of variation that crops up and insists on being recorded when the solar spectrum is photographed in anything like the detail which it absolutely demands. The method of recording will be at once understood.

Metal.	Wave-length.	Intensity in Sun. 1=Darkest.	Intensity in Photograph. 1=Brightest.	Intensity, Thalén. 1=Brightest.	
Manganese ...	4083 0	4	2	5	
	4083·5	3	2	3	
Iron	4197·5	1	2	–	
	4198·1	3	2	–	Stronger line.
Cobalt	4118·0	2	1	–	
	4120·5	4	1	–	Stronger line.
Nickel	4458·6	2	2	–	
	4647·8	3	2	5	
Chromium ...	4344·4	4	3	2	
	4351·8	3	3	2	
Molybdenum..	4706·5	3	1	4	
	4757·5	0	1	4	
Tungsten ...	4842·0	5	1	1	
	4887·5	3	2	2	
Titanium ...	3980·8	2	1	–	
	3989 25	1	1	–	
Zinc	4679·5	3	1	1	
	4721·4	4	1	1	
Platinum ...	4442·0	4	2	4	
	4551·8	3	2	2	
Palladium ...	3893·0	1	1	-	
	3958·0	3	1	–	Stronger line.
Zirconium ..	3957·22	2	1	–	
	3990·45	3	1	–	Stronger line.
Didymium ...	3989·65	0	1	–	
	3993·98	3	1	–	
Rubidium ...	4201·0	0	1	2	Stronger line.
	4215·5	3	1	0	

Now if Kirchhoff's statement be anything like a representation of the truth there ought not to be any difference between these intensities; the line least intense in the photograph ought to be least intense in Thalén's tables, and if it existed in the sun at all, it ought to be least intense amongst the Fraunhofer lines, but as a matter of fact, there is an absolute inversion. To take instances; the cobalt line 4120·5 is four times as intense in the photograph as in the sun; in the titanium line 3989·25 the intensities are equal; while in tungsten 4842·0 they are inverted, being represented as of minimum intensity in the sun, and of maximum intensity by Thalén and in the photographs. In the sun one of the lines of iron is given as of first, and the other as of third intensity, while in the photograph they are both of the second order. Again, in didymium we get a first order line recorded in the photograph which is absent from the sun altogether, whereas another line of the first order, near it, is there as a line of feeble intensity; so also in rubidium, and so we might go on.

2. *Coincident Lines in Different Spectra.*

The final reduction of all the photographs summarised all the observations of metallic spectra compared with the Fraunhofer lines accumulated during the whole period of observation, for the region lying on both sides of H and K. This reduction taught us that *the hypothesis that identical lines in different spectra are due to impurities is not sufficient.*

We found, with the dispersion employed (which had been greater than any employed before), short-line coincidences between many metals the impurities of which had been eliminated, or in which the freedom from mutual impurity had been demonstrated by the absence of the longest lines.

I will begin by directing special attention to what happened with regard to the spectrum of iron. We first mapped all the lines of iron observed on one of the photographs, including, of course, all impurities; finally we got rid of the impurities by the process which I have already explained, and at last we got what was called a purified spectrum, in which, along the horizon labelled iron, we had only those lines left which we could not by any application of the principle which has been explained show to be due to the admixture of any other substance whatever. What then was the total result? The accompanying table will show the sort of corner in which we found ourselves

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
1	39 0600 2	U 3, Zr 5, Yt 4
3	0622 4	Va 4
2	0920 3	Va 2, Ba 3
3	1010 4	Va 4, Pt 3
2	1648 2	Co 3
2	1755 3	Mn 3, Ce 4
2	1835 4	Os 2
1	2700 1	Va 2
1	2950 1	Mo 3
3	3023 4	Ce 4
5	3435 4	U 2
3	3475 2	Ba 2, Rh 2
3	3528 3	Ta 3
2	3975 2	Co 3
3	4026 3	Va 5
3	4422 4	Mo 3
3	4720 2	Yt 5, Th 3
2	5012 2	Di 2
2	5160 2	Ce 3, Ru 3
2	5210 3	W 3
2	5423 4	U 3, Mo 3, W 4
3	6215 3	Yt 5, Ce 3, Di 2
2	6571 2	Zr 2
3	6662 2	Th 1
1	7555 3	Os 2, Ta 4, Cr 2
3	7578 4	Di 2
2	7685 2	Va 4
2	8083 2	Ti 1
1	8320 1	Cr 3
3	9520 3	Ru 3
2	9750 2	Mo 3

after all this work had been accomplished. It gives a list of the iron lines in a small part of the spectrum, which, after making every allowance for the existence of impurities, we found to coincide with lines in other substances.

It will be seen, to take some instances from the above table, that the two short lines 390600 and 395423 coincided, the first with short lines in uranium, zirconium, and yttrium, the second with short lines in uranium, molybdenum, and tungsten. Similarly there are two short-line coincidences with zirconium, and no less than six with vanadium, and so on. The total gives the coincidences of the lines of all the elements under the conditions that I have drawn attention to. So that the sum total of this really very laborious inquiry with regard to iron was, *first,* that in the region between 39 and 40, the region including H and K, where, before the introduction of photography, scarcely any iron lines had been seen, and where only five solar lines had been given in Ångström's atlas, photography gave us a total of nearly 300 lines in the solar spectrum, and it gave us sixty-two lines of iron.

Next, *of those sixty-two lines of iron only eighteen were what we then considered normal;* by which I mean that the remainder had short-line coincidences with lines of other substances. So that the idea first thrown out by Kirchhoff, Ångström, and Thalén of the possibility of the coincidence of lines among the metallic elements seemed entirely endorsed. It will be seen that it is the rule in the case of iron, and it might be the case also in other substances. The fact of a line not being coincident with a line in another substance was the exception, and not the rule. The ratio in the case of iron being as 44 to 18 over the region examined.

I now proceed to give the results in the case of titanium. We got one case of three coincidences, five cases of two, and ten of one coincidence:—

FINAL REDUCTION—TITANIUM.

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
1	39 0000 3	Zr 4
4	0048 3	Th 4
5	1040 5	Mn 4 Ce 5 Di 3
2	1360 3	Va 4
5	1915 8	Ce 4
4	2050 3	U 3 La 3
3	2368 2	Va 3
3	3718 5	Th 4 Ce 4
2	4775 1	Fe 2
2	5722 1	Zr 1 3
4	6175 2	U 3
3	6335 2	Di 3 Ta 5
2	8083 1	Fe 2
3	8152 2	Mo 3
1	8922 1	Mn 4 Cr 4
2	9798 longest	Va longest

Since therefore these lines which were common to two or more spectra could not be traced to impurities, what was their probable origin? Their number was so great that to attribute them to physical coincidences, and to rest and be thankful accordingly, would have been to take the very pith and marrow out of the science of spectrum analysis, which we have heard so often is based absolutely upon different substances giving us spectra with special lines for each. The matter then was worthy of serious investigation.

Lines of one kind we could explain, *on the hypothesis that the elements are truly elementary*, by supposing that in the case, let us say, of coincident lines in the spectrum of iron and cobalt, the common line was due to an impurity either of iron in the cobalt or of cobalt in the iron. Most spectroscopic workers were of the true faith in this matter; they accepted the dicta of the chemist, and not only was the work which had shown how the phenomena observed *might be* thus explained received with favour, but no one, so far as I know, inquired whether there was any other "might be" in the matter.

The other set of lines was as different as possible. Of them there was, on the impurity hypothesis, no possible explanation forthcoming without changing ground. In fact, the separation of the coincidences into two classes was brought about by this very circumstance, since all the coincidences which, in accordance with a general law established for a constant temperature some years before, could be attributed to impurity had, as a matter of fact, been eliminated from the maps at a prior stage of the investigation. Further, be it noted that all the photographs represented the work of similar temperatures, for they were all taken with electric arcs, for the production of which the same number of Grove's cells was used in all cases.

It is amusing to go back to the old observation books, and to see with what pertinacity for the first two years we stuck to the possibility that the solar line or the metallic line we were dealing with was a double line, and then, after we had to give that idea up, as the coincidences became of three- four- five- and six-fold complexity, we came to the conclusion that we were dealing with a common impurity. That of course was a point we could not settle until we had gone through all the chemical elements which were known to us, and it was the discussion of the spectra of so many substances which took up so much time.

CHAPTER XVIII.

DISCUSSION OF THE DISSOCIATION HYPOTHESIS.

1. *Variation of Intensity.*

SEEING that such an abundance of new facts had now been collected, it really did seem time to discuss the question to which reference has already been made.

I approached it in the following manner, relying upon the spectroscopic behaviour of compound bodies already stated on p. 159, in order to keep as near to facts as possible:—[1]

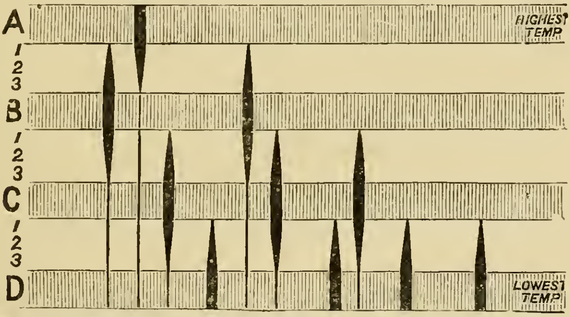

FIG. 86.—Hypothetical furnaces. The figures between the hypothetical spectra point to the gradual change in the intensities of the lines as the spectrum is observed near the temperature of each of the furnaces.

Let us assume a series of furnaces A—D, of which A is the hottest (Fig. 86).

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[1] *Proc. Roy. Soc.*, No. 191, 1878.

Let us further assume that in A there exists a substance a, competent to form a compound body β, by union with itself or with something else, when the temperature is lowered.

Then we may imagine a furnace, B, in which this compound body exists alone. The spectrum of the compound β would be the only one visible in B, as the spectrum of the assumed elementary body a would be the only one visible in A.

A lower temperature furnace, C, will provide us with a more compound substance γ, and the same considerations will hold good.

Now, if into the furnace A we throw some of this doubly-compounded body γ, we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β; finally a will exist alone, and the spectrum will be reduced to one of the greatest simplicity.

This is not the only conclusion to be drawn from these considerations. Although we have, by hypothesis, β, γ, and δ, all higher, that is more compound, forms of a, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the relative intensities of the lines in C and D are entirely changed.

Here is another diagram representing the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into a, is far below that stage, although higher than B.

It will be seen from the diagram (Fig. 87) that then *the only difference in the spectra of the bodies existing in the four furnaces would consist in the relative thicknesses of the lines.*

I confess that the result of this simple projection of what might happen to elementary bodies if they behaved like compound ones, struck me with surprise. It seemed to make everything so clear.

In the first place it presented us with such a simple and sufficient cause for the differences in intensities of lines, not only on passing from one temperature to another in the laboratory, to which Kirchhoff and Ångström were the first to refer, but also on passing from laboratory to sun.

It was in 1874 that I first glimpsed the idea that the line spectrum of a substance was probably produced by molecules of different degrees of fineness into which the substance was driven by the temperature employed.[1]

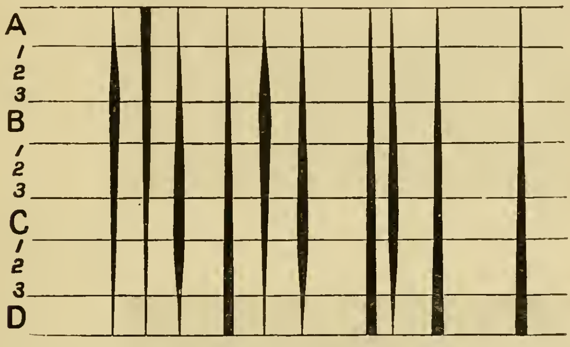

FIG. 87.—Hypothetic furnaces. In this case the temperature of A is not so great as in the former one.

Now these molecules of different finenesses are represented by α, β, γ, and δ, in the hypothetical diagrams given above; indeed, it is quite easy to see that, if we change the temperature of the furnaces in such a manner as to produce in turn the lines of each of these hypothetical molecules in their most intense and wide condition owing to the great quantity of vapour rich in that particular kind of molecule, the strong lines produced at these different temperatures would vary; the strongest line produced in furnace D would not be the same as the strongest line produced in furnace A; so that in that way we can imagine a very high temperature giving a very strong line in the spectrum of a particular substance, which may yet at a lower temperature only appear as an exceedingly feeble one.

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[1] *Proc. Roy. Soc.*, vol. xxii. p. 380, 1874.

Evidence furnished by Calcium, Magnesium, and Lithium.

This being premised, let us now go back to page 194, and study what was there said regarding the variation of spectral lines from this new point of view. I there gave the facts touching the variations in the spectra of calcium, magnesium, and

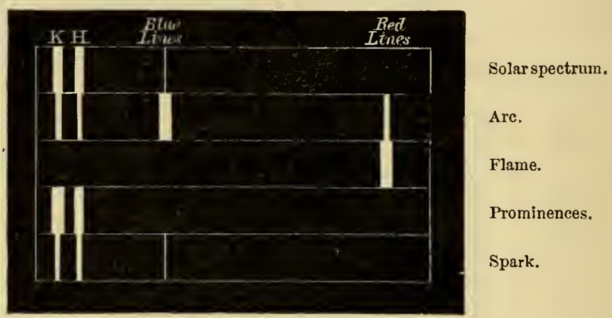

FIG. 88.—The varying intensities of the lines of Calcium as seen under different conditions.

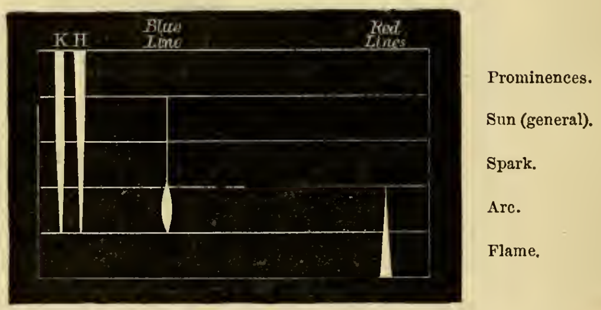

FIG. 89.—The varying intensities of the lines of Calcium with increasing temperatures.

lithium. I reproduce Fig. 75. The question is, can we arrange these spectra along any one line? and if so, does this line represent a change of temperature merely? Fig. 89 shows the result of this inquiry. It will be seen how in all essentials it reproduces our hypothetical diagram, Fig. 87.

The effect of temperature on the spectrum of calcium may be stated as follows:—

(1) At a low temperature we get a spectrum of calcium which contains no lines whatever in the blue; (2) When we increase that temperature—the temperature of a Bunsen burner is sometimes sufficient—we get a line in the blue at wave-length 4226·3. (3) When we pass from a Bunsen burner to an electric lamp we get this blue line intensified, and at the same time we get two new lines in the violet, named H and K, at wave-lengths 3933 and 3968. (4) Using a still higher temperature in the arc, we thin the blue line, and at the expense of that line, so to speak, we thicken the two in the violet, so that the latter equal the blue line in thickness and intensity. (5) Passing to a large induction coil with a small jar we make the violet lines very much more prominent; (6) and using a larger induction coil and the largest jar we can get, we practically abolish the blue line and get the violet lines alone. Note that we have simply produced these effects by varying the temperature.

Figs. 90 and 91 enable us to see what happens in the case of magnesium; how disorder is at once abolished, and how exactly the same order is brought about by the same progression as in the case of calcium, from flame through arc, spark, sun (general) to prominences, the last term in both series.

Next comes the case of lithium (Figs. 92 and 93).

As before, we find order brought about and the hypothetical diagram reproduced by the same series of increasing temperatures, but here the last term in the series is wanting, for so far no line of lithium has been seen bright in the prominences.

That in the case of the blue line we are really dealing with an effect of temperature, long ago recognised, may be seen from a notice of this lithium line in a discourse by Professor Tyndall, reprinted in the *Chemical News*, and in a letter of Dr. Frankland's to Professor Tyndall, dated November 7, 1861. This

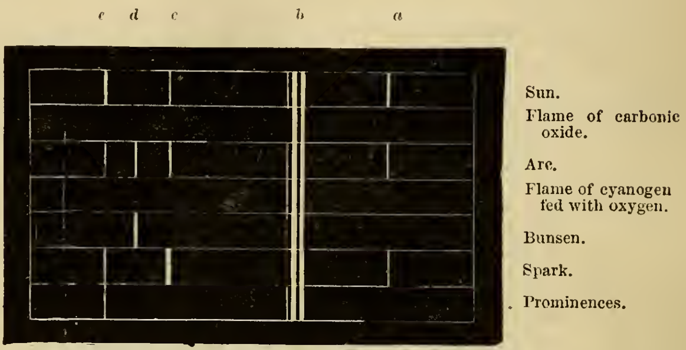

FIG. 90.—The various intensities of the lines of Magnesium as seen under different conditions

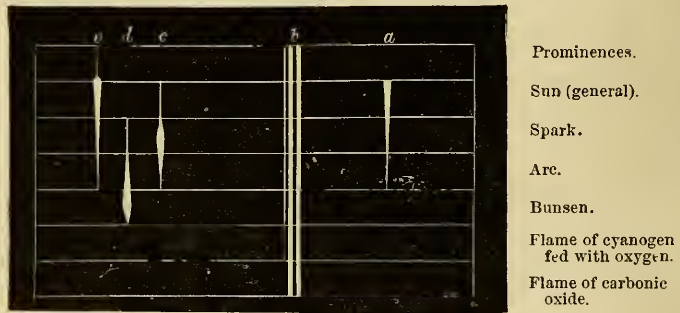

FIG. 91.—The various intensities of the lines of Magnesium arranged in order of increasing temperatures.

letter is so important for my argument that I reprint it entire from the *Philosophical Magazine*, vol. xxii. p. 472.

"On throwing the spectrum of lithium on the screen yesterday, I was surprised to see a magnificent blue band. At first I thought the lithic chloride must be adulterated with strontium, but on testing it with Steinheil's apparatus it yielded normal results without any trace of a blue band. I am just now reading the report of your discourse in the *Chemical News*, and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the carbon points or ignited air guilty of its production? I find these blue bands with common salt, but they have neither the definiteness nor the brilliancy of the lithium band. When lithium wire burns in air it emits a somewhat crimson light; plunge it into oxygen, and the light changes to bluish white. This seems to indicate that a high temperature is necessary to bring out the blue ray."

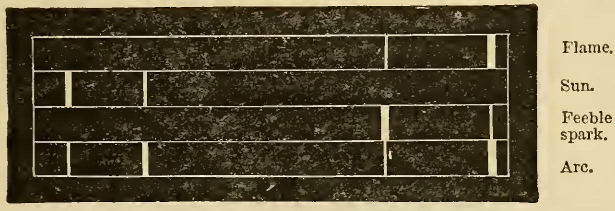

FIG. 92.—The various intensities of the lines of Lithium under different conditions.

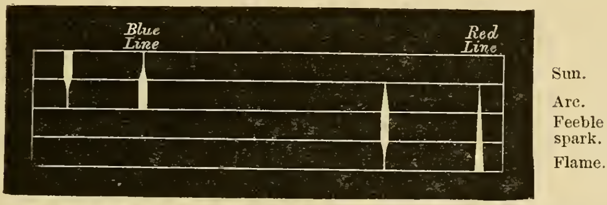

FIG. 93.—The various intensities of the lines of Lithium arranged in order of increasing temperatures.

Postscript, November 22, 1861.—"I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line depends entirely on the temperature. The spectrum of lithic chloride, ignited in a Bunsen's burner flame, does not disclose the faintest trace of the blue line; replace the Bunsen's burner by a jet of hydrogen (the temperature of which is higher than that of the Bunsen's burner) and the blue line appears, faint, it is true, but sharp and quite unmistakable. If oxygen now be slowly turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum, and thus put an end to the experiment."

It is remarkable that in the case of this body which goes through its changes at relatively low temperature, its compounds are broken up at the temperature of the Bunsen burner. Thus the spectrum of the chloride, so far as I know, has never been recorded.

Stellar Evidence.

I propose now to return to the case of calcium and see how the views put forward on solar and terrestrial evidence are borne out by the facts which are presented to us by the stars. There is no need to occupy much space in this, in fact reference need only be made to Dr. Huggins's paper which was communicated to the Royal Society in 1880, and with that paper we may compare some earlier writings. It was as early as 1864 that Dr. Huggins, who was then associated with the late Prof. Miller, called attention to the strong lines of hydrogen visible in the spectra of the hottest stars.[1] In this paper it was pointed out at the same time that other metallic lines associated with those lines of hydrogen were thin and faint. It has been already mentioned that, as we have independent evidence that these stars are hotter than our sun, we had strong grounds for believing that here we were in presence of a result brought about by a higher temperature, associated as it was with a simpler spectrum, and, therefore, presumably with simpler constituents.

We need not stop now to discuss the objection which has been put forward by an ingenious person ignorant of the facts, that the broadening of these lines may not be due to an increase of temperature at all, but really to a very rapid equatorial rotation of the star. This is a fair sample of one of the classes of objections one has to meet. Of course it is at once put out of court by the fact, also stated by Dr. Huggins, that, associated with the thick lines, are excessively thin ones. Any enormous equatorial velocity of the star should have made all the lines thick, or have obliterated them, but this is not so. Now we have only two lines in the solar spectrum at all comparable in thickness with these hydrogen lines in the hottest stars, taking Sirius and *α* Lyræ as types.

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[1] "On the Spectra of some of the Fixed Stars," *Proc. Roy. Soc.* 1864, p. 242.

In a paper communicated to the Royal Society in 1876[1] I remarked that laboratory work indicated the possibility that line-spectra might, after all, really not result from the vibration of similar molecules; and at that time the evidence seemed to be so clear in the case of calcium that it was pointed out that the time had arrived when evidence touching the H and K lines of that substance ought, if possible, to be obtained from the stars by means, of course, of photography, because the part of the spectrum in question is exceedingly faint in the case of the stars.

Why, it may be asked, was it important to get this evidence from the stars? I here give an extract from a book,[2] published some years ago, which puts this view forth :—

> "It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms, those that give us line-spectra, are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and, therefore, no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other similar atoms at all lower temperatures. Hence the association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end."

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[1] "Preliminary Note on the Compound Nature of the Line-Spectra of Elementary Bodies," *Proc. Roy. Soc.* No. 168, 1876.

[2] *Studies in Spectrum Analysis*, p. 196.

That was one point.

Here is the next point which made an appeal to the stars so necessary.

> "We are justified in supposing that our terrestrial calcium once formed is a distinct entity, whether it be an element or not, and, therefore, by working at terrestrial calcium alone we shall never know, even if its dissociation be granted, whether the temperature produces a simpler form, a more atomic condition, of the same thing, or whether we are able to break it up into $x+y$, because in our terrestrial calcium, assuming all calcium to be alike, neither x nor y will ever vary; but if calcium be a product of a condition of relatively lower temperature, then in the stars hot enough to enable its constituents to exist uncompounded, we may expect these constituents to vary in quantity; there may be more of x in one star and more of y in another; and if this be so, then the H and K lines will vary in thickness, and the extremest limit of variation will be that we shall only have H, representing, say, x in one star, and only have K, representing, say, y in another. Intermediately between these extreme conditions we may have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others."

What, then, is the result of my appeal to the stars which Dr. Huggins's beautiful researches have rendered possible? We have in the hottest stars a spectrum so regular, so rhythmic, that it seems impossible not to consider it as produced either by the same substances or by substances closely allied. Is it by mere accident that some of the *least* refrangible lines coincide with those of hydrogen? Fig. 94 is a copy of Dr. Huggins's diagram, to which reference has been made. At the top is a portion of the solar spectrum in the violet and ultra-violet, and next is the spectrum of the hottest star, *a* Lyræ. This spectrum, it will be seen, is simpler even than the spectrum of the solar prominences, and not only is there this wonderful simplicity, but an exquisite rhythm by which the distance between the lines gradually increases as we go from one end of the

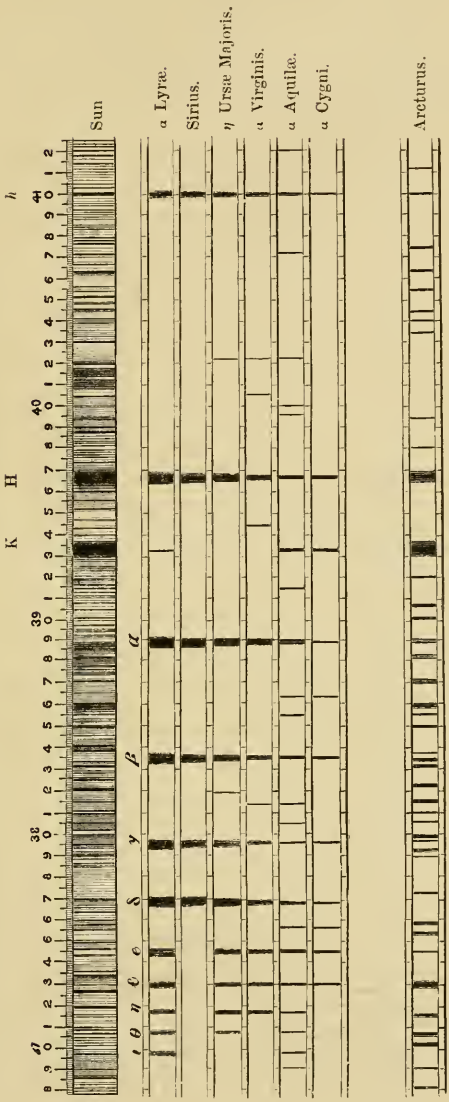

Fig. 94.—Stellar spectra (Huggins). In this diagram the spectrum of the sun is given at the top so that the spectra of the stars can be compared with it. The spectra of the stars are reduced from photographs, and the order of arrangement has been determined by the gradual thinning of the chief lines.

spectrum to the other. Note also that the least refrangible line shown on the diagram is coincident with *h* in the violet part of the solar spectrum, and that the next line is coincident with the line H, to which reference has been made in the notes I have quoted. Note also the relative intensities of the lines H and K in the sun, in which their intensities are about equal, and in η Ursæ Majoris, in which K is altogether absent. These are the first points in this diagram to which attention must be drawn. There will be other points as we proceed further.

In descending from the general to the particular Dr. Huggins writes:[1]—"The spectrum of Vega may be taken conveniently as typical of the whole class of white stars, so that the distinctive features of the other stars of this class may be regarded as modifications or departures from this common typical form." He then adds: "There are principally three directions in which changes take place;" one of these consists "in the presence or absence of K, and if present, in its breadth and ntensity relative to H." He goes on, "One of these modifications, which possess great suggestiveness, consists of the absence, or difference of character presented by the line K. In all the stars of this class this line is either absent or is very thin as compared with its appearance in the solar spectrum, at the same time that H remains very broad and intense. In the spectrum of Arcturus, a star which belongs to another class which includes our sun, this line K has passed beyond the condition in which it occurs in the solar spectrum, and even exceeds the solar K in breadth and intensity." Arcturus is given in the lower part of the diagram, and it will be seen that there K is relatively thicker than H; and also that with this relative increase in the thickness of K we get a considerable complexity of spectrum, very much more approaching the solar spectrum in the number of lines that we have to contend with. But at the same time I should point out that the positions of these lines vary from the positions of lines in the solar spectrum. "The spectra of these stars," Dr. Huggins continues, "may therefore be arranged in a continuous series, in which first we find this line to be absent. Then it appears as an exceedingly thin line. We then pass to another stage in which it is distinct and defined at the edges; in the solar spectrum it becomes broad and winged, and lastly in Arcturus there is further progress in the same direction, and the line, now a broad band, exceeds in intensity H." Absolute continuity we see is the story which Dr. Huggins brings us with reference to this concrete case of H and K in the details and in the general. Well might Dr. Huggins ask after this: "Do these modifications not represent some of the stages through which our sun has passed?" In another part of his paper he uses the term "life changes." Now the life of a star is its temperature, and all these changes must have been produced by the running down of temperature, and I think the simplest view to take, limiting ourselves to the concrete case of H and K, is that the substance which produces K has been formed at the expense of the substance which produces H, and the reason that we see these two lines in calcium when a high temperature is employed is that we reveal the presence of these true root-forms. There may be very many more difficult explanations, but that I think is the simplest one to which one is driven by the logic of facts.

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[1] *Phil. Trans.* 1880.

The appeal to the stars, then, I think, amply justifies the prediction which I based upon the comparison of solar with terrestrial phenomena; and, therefore, tends to show that the basis on which those predictions were founded had at all events some little glimmering of truth about it. I think also that it increases the number of dissociation stages through which we must assume the vapours of our so-called elements to pass when higher temperatures are employed in succession.

How rigid a test we have been able to apply to these views by means of Dr. Huggins' remarkably beautiful researches the accompanying woodcut, Fig. 95, will show; a perfect continuity exists between the old facts and the new; the stars arrange themselves along the same line as do our terrestrial heat sources.

The wide departure of stars hotter presumably than the sun (taking the absorption of the rhythmic series of lines, as the indication of temperature) from the solar type shows that there is much more work to be done in this field. The success of my former prediction emboldens me to make another one.

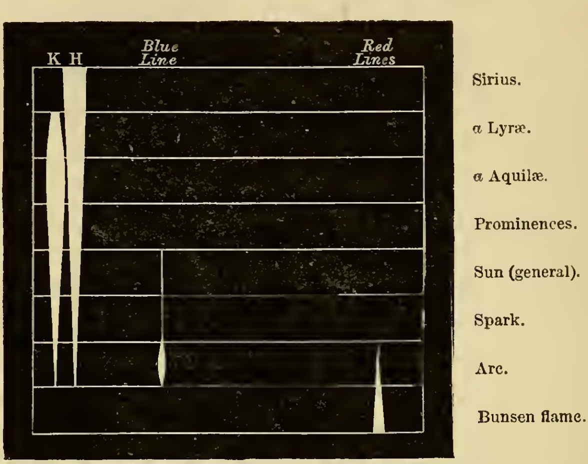

Fig. 95.—The changes in the spectrum of Calcium from the Bunsen flame to Sirius.

It will in all probability be found that the remaining thick lines in stars of the Sirius type are represented in many cases by the lines brightened in solar prominences.

2. *Lines in the spectra of two or more substances.*

We have now to point out another direction on which the discussion of the hypothetical furnaces seemed to throw some light. The next conclusion was quite unexpected. Let us take the conditions represented in Fig. 87, and remember that in almost all cases the strongest line in a spectrum at any one temperature is the longest. Although the spectrum of the substances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; each line would be thick at one temperature and thin at another. It would be therefore longest at one temperature and shortest at another. Hence, since *the longest lines at one temperature will not be the longest at another, the whole fabric of "impurity elimination," based upon the assumed single molecular grouping falls to pieces.*

To take a concrete case. Let us suppose that in Fig. 87 the four furnaces represent the spectra of iron broken up into different finenesses by successive stages of heat. It is first of all abundantly clear that the relative thicknesses of the iron lines observed will vary as the temperature resembles that of A, B, C, or D. The positions in the spectra we may assume to be the same, but the intensities will vary; this is the point. The strongest, and, therefore, as a rule the longest, lines will vary as we pass from one temperature to another.

But how does the whole fabric of impurity elimination fall to pieces? In this way.

Let us suppose that manganese is a compound body, and that one of its constituents is a form of iron represented in furnace B. Suppose the photograph of the spectrum of iron I compared with that of manganese is taken at the temperature represented by furnace D, and that the photograph of manganese is also taken at the same temperature; now, to eliminate the impurity of iron, in the manganese, we look for the longest (strongest) lines in the iron photograph among the manganese lines. If we do not find them we say there is no impurity of iron; but although the longest lines are absent we get the shorter familiar ones.

It will be seen at once, then, that the consideration of the question gave as a natural consequence precisely the state of things detailed in page 232 *et seq.*, which on the old view seemed so inexplicable.

This was one way of regarding the matter, there is another which perhaps is more simple. The analogy between simple and compound bodies which we have heretofore employed, has been that suggested by the manner in which, after a compound of calcium and chlorine has been exposed to the action of a sufficient temperature, the spectra of calcium and chlorine, become visible in addition to that of the compound.

Now suppose we expose compounds of calcium with chlorine, bromine and iodine to a like temperature, we eventually get the calcium lines visible in all as the compound is broken up.

This, then, furnishes us with a new analogy, since the common lines seen in the spectra of elementary bodies may be likened to the lines of calcium, common to all the salts of calcium when they are dissociated.

I am particularly anxious to point out that there was absolutely nothing new imported into the consideration of the question. We had simply with regard to change of intensity taken as our guide the behaviour of a known compound body, and then pushed the reasoning three or four stages further. We had gone just the safest possible way, by the easiest possible stages, from the known to the unknown. This had brought home to us another analogy furnished by a group of compound bodies with a common base. No new theory in fact was necessary. The appeal to the law of continuity was open to us, and it seemed to answer our question at once.

What then was the first thing to be done?

In the first part of our inquiry we confined ourselves to the lines seen in the spectrum of one substance. It was obviously now our duty to study the lines seen in the spectra of different substances equally *au fond*.

First, it was of importance to see whether an examination of photographs gave us similar results with regard to the coincidence of lines in other parts of the spectrum than that first reduced. The question of *absolute* coincidence being reserved, as

I always have reserved it, we have to consider whether the lines are massed in certain regions of the spectrum. A rapid survey was made with this object, the result showing that the same thing did truly apply to these other regions with the dispersion we had employed.

Still full of the "solar line double" and "common impurity" point of view, the attack was varied by an inquiry whether there was any special character connected with the coincident lines. In this way it was imagined that some light might be thrown upon the question as to whether they were chance coincidences. In short, the thing done was inquiring whether these coincident lines varied their behaviour in some other special manner from non-coincident lines taken at random.

Supposing them to represent mere chance coincidences—"physical coincidences," as they have been called, or again, lines so near together that our means cannot separate them—there is no reason why they should behave differently from the other lines in a spectrum when the temperature is changed; while, as our hypothetical furnaces had taught us, if they be truly common, they *must* vary with temperature. Further, they must vary in such a way that other conditions being equal, they shall become stronger when the temperature is increased, and fainter when the temperature is reduced.

Now what was the best mode of attacking this problem? I was unable to see a more expeditious one than that presented to us by the sun's atmosphere. The following considerations will show how we might hope for help in this quarter.

Whatever be the chemical nature of this atmosphere, it will certainly be hotter at bottom—that is, nearer the photosphere—than higher up. Hence, if temperature plays any part in moulding the conditions by which changes in the resulting spectrum are brought about, the spectrum of the atmosphere close to the photosphere will be different from that of any higher region, and therefore from the general spectrum of the sun, which practically gives us the summation of all the absorptions of all the regions from the top of the atmosphere to the bottom.

Now as a matter of fact we have the opportunity, when we observe the spectrum of a sun-spot or a prominence, of determining the spectrum of a practically isolated mass of vapours *in the hottest region open to our inquiries,* and seeing whether it is like or unlike the general spectrum of the sun. What then are the facts ?

It is as unlike as possible : the intensities of the lines, as we have seen, are inverted to a wonderful extent. More than this : there is a constant difference between the spectra of sun-spots and the spectra of metallic prominences, though we see these phenomena generally at about the same *niveau* in the sun's atmosphere.

To get an idea of this inversion, maps were prepared of the spectra of the chief chemical substances showing the behaviour of the various lines under the various conditions. The result is very striking ; indeed, it is striking to quite an unexpected degree. The whole character of the spectrum of iron, for instance, is changed when we pass from the iron lines seen among the Fraunhofer lines to those seen among the spot and prominence lines ; a complex spectrum is turned into a simple one, the feeble lines are exalted, the stronger ones suppressed almost altogether.

Since then the spectra of spots and prominences are confessedly the spectra of the hottest regions of the sun available for our inquiries, we can test the nature of the common lines by seeing how they behave when we pass from the general solar to these special solar spectra.

With special reference to this point the various observations recorded of the lines visible in solar disturbances at the sun's limb, and of those observed to be widened, brightened or otherwise modified in the spectra of solar spots were confronted.

The finest series of observations of this kind that we possessed in 1878, was that collected by Prof. Young near the time of the last maximum of sun-spots, during his stay at Sherman, at a height of 8,000 feet. The result which stares us in the face when we examine the work done by Young is most striking; but although his observations of the chromospheric lines extend over the whole visible spectrum, the list of lines in the solar spots is limited to the less refrangible region; the discussion was therefore limited to this region.

As a basis for this discussion, the lines given in Thalén's admirable tables were taken, comparing them with those shown in Ångström's map, and indicating the intensities of the lines which are given in the tables, and which particular line occurs in the map only. A glance, then, shows which line is seen in spots and prominences, and how it is affected. In short we have in one view, for each metallic substance, exactly what happens to the lines of that substance—which lines are not touched; those which are visibly affected both in spots and storms, or those recorded in one table and not in the other.

Taking all the lines included in the discussion, the following statistics will show how they are distributed:—

Total number of lines in Thalén's list and map included in the discussion	345
Number of lines affected in spots	108
Number of lines bright in storms (prominences)	122
Number of lines common to spots and storms .	68
Number of lines seen in neither spots nor storms	183

So much for the list of lines as a whole. It is also necessary to show the number of lines assigned to each metal, and those among them which occur in both spots and storms, or only in one or the other. In order that this may be clearly shown the table overleaf is appended.

Metal.	Number of lines.	Number of lines common to spots and storms.	Number of lines due to		Unaffected.
			Spots.	Storms.	
Sodium	8	4	6	6	0
Magnesium ...	4	3	4	3	0
Barium	23	1	3	7	14
Calcium	25	7	15	10	7
Strontium ...	18	0	0	0	18
Nickel	12	1	3	2	8
Cobalt	19	3	3	3	16
Manganese ...	16	2	3	6	9
Cadmium ...	15	0	0	0	15
Chromium ...	14	3	3	5	9
Titanium... ...	87	11	18	18	62
Iron...	104	33	50	62	25
	345	68	108	122	183

It will be seen that the ratio between the affected and unaffected lines is very variable. What strikes one, indeed, is the wonderful irregularity in the behaviour of the various lines; there is no relation, for instance, between the widening of the lines in the spots and their appearances in the prominences.

It may here be asked, "But what has this to do with the lines common to two or more spectra?" I answer, it would have nothing to do with such lines if Thalén had not observed them; but in his observations, which are the *ne plus ultra* of spectroscopic accuracy, he came across them abundantly.

Among the 345 *lines given by Thalén are* 18 *with identical readings in two spectra.* They are, therefore, the exact equivalents of those lines found to be coincident in work on another part of the spectrum.

Now, for the reasons above given, if the explanation of their basic character suggested by the consideration of the hypothetical furnaces be the correct one, then we should expect a considerable development of these lines in the spectrum of the hottest regions of the sun, which spots and storms enable us to study apart from the absorption going on at higher levels.

It is not too much to say that the result of this inquiry was most striking. What came out in the strongest manner was the very remarkable fact that *these common lines were always widened in the spots.* However feebly the brighter lines of a chemical substance, taken as a whole, might be represented amongst the spot lines, yet the common lines among these, which are often of the second or third order of intensity and sometimes even of the fourth, are never absent. The same fact held almost equally true with regard to the storms.

The comparison of Thalén's lines, recorded in two spectra, with those seen by Young in solar spots and storms shows this result:—

THALÉN.			YOUNG.		
Wave-length.	Common to	Intensity.	Spots.	Storms.	
			Widening.	Frequency.	Brightness.
5207·6	Fe Cr	3 1	4	10	6
5203·7	Fe Cr	3 1	4	10	6
5340·2	Fe Mn	2 3	2	1	2
6064·5	Fe Ti	2 2	3	5	2
5661·5	Fe Ti	3 1	4	15	2
5403·1	Fe Ti	2 3	4	5	3
5396·1	Fe Ti	2 2	7	4	2
5352·4	Fe Co	4 3	2	4	2
5265·8	Fe Co	2 3	2	10	4
5168·3	Fe Ni	3 5	4	40	30
5166·7	Fe Mg	2 1	2	30	20
5681·4	Fe Na	3 3	3	2	1
6121·2	Co Ca	1 3	4	5	3
5601·7	Ca Fe	4 1	2		
5597·2	Ca Fe	3 1	2		
5856·5	Ca Ni	3 4	2		
5425·0	Ba Ti	3 3	4		
6449·0	Ca Ba	2 3	2		

So far as my own knowledge of these matters goes, I can imagine no severer test to apply to the hypothesis that the coincident lines in the above table are not produced by chance.

We may conveniently sum up what has been advanced in this chapter as follows :

Attempting to picture the spectral changes brought about by the action of a series of furnaces, we find that exactly such changes of intensity in spectral lines as have been recorded must be produced if dissociation takes place. This holds true, not only for metallic vapours, but for stars ; and some predictions which have been made on the strength of the hypothesis have been verified. Further, a continuation of the discussion of the hypothetical furnaces seems to give a *vera causa* for coincident lines, on the assumption that some of the elements have common bases, and a further consideration of the common lines observed indicate that they do not appear to depend upon the presence of impurities, and it is also shown that these coincident lines do somehow differ in other ways from non-coincident lines taken at random from the same spectra.

CHAPTER XIX.

DISCUSSION OF THE DISSOCIATION HYPOTHESIS.—*continued.*

It will, I think, be generally conceded that the results referred to in the preceding chapter, gave us *prima facie* ground for thinking that it was quite worth while to go on with the consideration of the hypothesis which had been advanced.

We saw that dissociation did seem to explain the changes in the intensity of lines of spectra on the analogy of the dissociation of known compounds.

I was careful at the very outset to point out that the view advanced is based upon the analogies furnished by those bodies which by common consent, and beyond cavil and discussion, are compound bodies. Indeed, had I not been careful to urge this point, the remark might have been made that the various changes in the spectra to which I have drawn attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell, with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular grouping of hydrogen as that which gives us a green line only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green line of hydrogen without the violet one. Hence the argument founded on the overnotes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures, is as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other.

We next saw that common lines had been observed in the spectra of various substances under conditions which seemed to put impurity out of the question, and that then another line of inquiry had been undertaken to see if we were not after all dealing with apparent coincidences only. The result of this inquiry was to show that the coincident lines really behaved differently in the sun from the non-coincident ones.

The view of which we have now to continue the consideration, is, I think, after all but a slight expansion of the present chemical view. Chemists regard matter as composed of atoms and molecules. The view now brought forward simply expands the series into a larger number of terms, and suggests that the molecular grouping of a chemical substance may be simplified almost without limit if the temperature be increased. A diagram (Fig. 96) will show exactly what I mean, and what, in fact, flows easily from the consideration of the hypothetical furnaces referred to in a preceding chapter. If we assume a very great difference in the temperature which can be brought to bear upon a substance, we may assume that at the highest temperature we have, for simplicity's sake say, a single line represented by a single circle; let us imagine the temperature reduced, we shall then get another spectrum, which we can represent by a double circle, if we like to assume that the evolution is one which proceeds by constant additions of the original unit. Coming lower down, we get another substance formed with a more complex spectrum represented by three circles; lower down still we have one represented by four circles, another by five, another by six, and so on. We might take another supposition, easier perhaps to some minds, and suppose that evolution proceeded, not by the constant addition of the initial unit, A + A, but by the constant doubling of the substance of the molecule itself. Instead, therefore, of our circles increasing by one, we shall have one, two, four, eight, sixteen, thirty-two, and it will be readily understood that if there are a considerable number of stages of temperature, both within our ken and beyond our ken, and if some substances form themselves perpetually by doubling, then the unit with which we can experiment at low temperature, call it the chemical atom

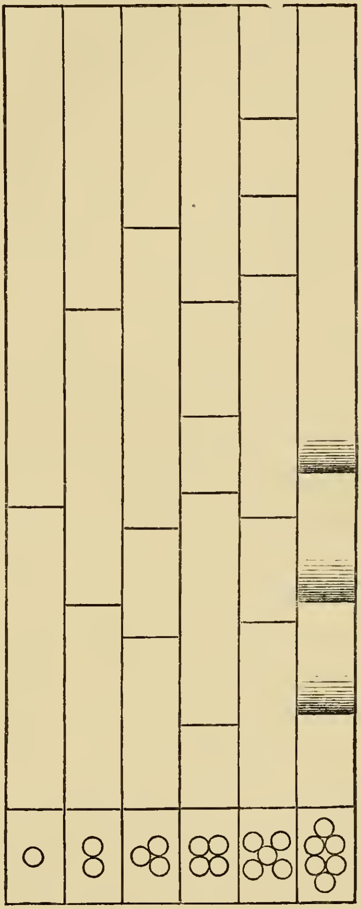

Fig. 96.—Diagram showing how the evolution of chemical forms may be indicated by their spectra.

or the chemical molecule, or what you will, must be a very complex thing indeed. If the lower spectrum represents that of a complex body such as iron, or a salt of calcium, the upper spectra will represent those due to the finer groupings brought about by higher temperatures. We pass continuously, as in the sun and the stars, from complexity to simplicity, if we begin at the lower, and from simplicity to complexity if we begin at the higher, stages of temperature.

As it is most important to obtain a clear mental view of the manner in which, on the principles of evolution, various bases may be formed, it is as well to point out here that although it does not seem unnatural that the bases should increase their complexity by a process of continual addition of like units, or even by continual multiplication of them, the factors being 2, 3, or some higher number; still that these need not necessarily, or even probably, be the only lines. Thus we may have increase of molecular complexity by the addition of molecules of different origins, and seeing that we must suppose these early forms to be produced in a mass of incandescent vapour, probably in a state of the wildest commotion, I should fancy that the greater number after the first would be formed in such fashion. $A + A$ would give place to $A + B$, and a variation of the process would consist in a still further complexity being brought about by the addition of another molecule of B, so that instead of $(A + B)_2$ merely, we should have $A + B_2$.

Now, two questions arise here which I think it is important to discuss. Are we playing fast and loose, in such an hypothesis as this, with the ordinary course of nature's operations, or are we in harmony with her? Again; is it contrary to the view expressed by the greatest minds which have studied chemical phenomena? That the view is not inharmonious with the theory of evolution which has been formulated, or other view which we have gathered from other regions of thought and work, is at once obvious; in fact, I think it derives its whole force from the fact that along many lines it runs parallel with the processes of development in the different kingdoms of nature. Of course we know that in the organic kingdom evolution always proceeds along many lines; in the inorganic world the environment was less complex, but to simplify the problem it is permissible to take one of the simplest that I can think of.

Let us assume that in a certain very hot star there shall be two substances, which we will call a and b. They will first at the transcendental temperature which I assume, exist as separate entities. The temperature being then reduced, they probably will combine, and, instead of two atoms, a and b, we shall have one group of $a + b$. If the temperature is still further reduced, we shall get b combining with b; along that line we shall have a grouping consisting of $a + 2b$. Let the same operation be performed again, and we shall then have $a + 4b$; we shall have what we can represent, in short, in chemical language by ab_4. Now, having got our ab_4,—having got our temperature reduced, let us assume that ab_2 is now the substance linked on to give a greater complexity, instead of b or $2b$ merely. We then have this series given in the table.

TABLE I.

HYPOTHETICAL SERIES.

$$
\begin{array}{rl}
 & a \qquad b \\
 & a + b \\
 & a + bb \\
 & a + (bb)(bb) \\
 & \quad = \ldots ab_4 \\
+ & ab_2 = \ldots\ldots a_2b_6 \\
+ & ab_2 = \ldots\ldots\ldots a_3b_8 \\
+ & ab_2 = \ldots\ldots\ldots\ldots a_4b_{10} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots a_5b_{12} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots a_6b_{14} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots a_7b_{16} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_8b_{18} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_9b_{20} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{10}b_{22} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{11}b_{24} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{12}b_{26} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{13}b_{28} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{14}b_{30} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{15}b_{32} \\
+ & ab_2 = \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a_{16}b_{34}
\end{array}
$$

Now, that is an ideal scale. The question is, Is it absurd? How can we honestly answer that question? By asking whether we are or are not on the lines on which nature works in the region of the known—in the region which we can get at.

We will now refer to another table; we will pass from the ideal to the concrete, and it will be seen that there is, if one can invert the term in such a way, a distinct precedent for such a table as the last; for here are the absolute facts with regard to one series of the combinations of carbon and hydrogen.

TABLE II.

CONCRETE SERIES OF HYDROCARBONS.

$C \quad H$
$C + H$
$C + HH$
$C + (HH)(HH)$
$= ...CH_4$
$+ CH_2 = ...C_2H_6$
$+ CH_2 = ...C_3H_8$
$+ CH_2 = ...C_4H_{10}$
$+ CH_2 = ...C_5H_{12}$
$+ CH_2 = ...C_6H_{14}$
$+ CH_2 = ...C_7H_{16}$
$+ CH_2 = ...C_8H_{18}$
$+ CH_2 = ...C_9H_{20}$
$+ CH_2 = ...C_{10}H_{22}$
$+ CH_2 = ...C_{11}H_{24}$
$+ CH_2 = ...C_{12}H_{26}$
$+ CH_2 = ...C_{13}H_{28}$
$+ CH_2 = ...C_{14}H_{30}$
$+ CH_2 = ...C_{15}H_{32}$
$+ CH_2 = ...C_{16}H_{34}$

We have gases CH_4, C_2H_6, C_3H_8; followed by liquids from C_4H_{10} to $C_{15}H_{32}$; each of them formed, not by the addition of my hypothetical ab_2, but by a concrete CH_2, and we have connected with that a beautiful order and exquisite regularity in the way in which the boiling-points and specific gravities of the successively more complex forms increase, as will be seen in the next table.

TABLE III.

HYDROCARBON SERIES.

			Boiling point.	Specific gravity.
Gaseous	CH_4	Marsh Gas.		
	C_2H_6	Ethane.		
	C_3H_8	Propane.		
Liquid ...	C_4H_{10}	Tetrane or Diethyl	1°	·600 at 0°
	C_5H_{12}	Pentane	38	·628 at 17
	C_6H_{14}	Hexane or Dipropyl	71	·669 at 16
	C_7H_{16}	Heptane	99	·699 at 15
	C_8H_{18}	Octane	124	·726 at 15
	C_9H_{20}	Nonane	148	·728 at 13·5
	$C_{10}H_{22}$	Decane	166	·739 at 13·5
	$C_{11}H_{24}$	Endecane	180	·765 at 16
	$C_{12}H_{26}$	Dodecane or Dihexyl.	202	·774 at 17
	$C_{13}H_{28}$	Tridecane	218	·792 at 20
	$C_{14}H_{30}$	Tetradecane	230	—
	$C_{15}H_{32}$	Pentadecane	258	·825 at 16
Solid ...	$C_{16}H_{34}$	Hexdecane or Dioctyl	278 Melts at 21°	

There is no break in the general line of increase, and after we have gone through the gaseous stage, which stops at C_3H_8, and through the liquid stage, which stops at $C_{15}H_{32}$, we get the solid state, and there again the same series is represented. So that I think one is justified in saying that, dealing with this one simple case (and the only reason why I have taken it is that it is a line which has been thoroughly worked out by organic chemists), we are justified in saying that if nature, in the regions which we cannot get at, works in the same way as she does in the regions which we can get at, the view is not absurd, and in fact any one who wishes to dispute it in such a case as this has, I think, the *onus probandi* thrown upon him. He must show that either in a certain latitude or longitude, or at a certain temperature, or under some unknown condition the laws of nature are absolutely changed, and give place to new ones. That has not yet been found in any other region of natural philosophy. Indeed I think one might go further and say that all the processes of development observed in different regions of thought, have such a oneness about them that to my mind one of the best mental images we can get of the conditions which determine the lines picked out for special prominence in solar spots and solar flames, is to consider the molecular groupings that produce them as resembling the roots of the present European languages which our ancestors brought from the cradle of the race in Asia.

Now comes the second question, to which reference has been made. What is the opinion of those who have given the greatest attention to chemical philosophy? I do not mean to chemistry, I mean to chemical philosophy. I have already referred to Brodie's clear statement, but we can anticipate his time by a reference to Dalton. He says, "We do not know that any one of the bodies denominated elementary is absolutely indecomposable." Graham also wrote, "It is conceivable that the various kinds of matter now recognised in different elementary substances may possess one and the same element or atomic molecule existing in different conditions of movability. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies." The greatest chemical philosopher now living, M. Dumas, so long ago as 1836 published a series of lectures in which his views were very clearly stated indeed, and any one who reads them will see how convinced he was then of the considerable amount of evidence that had already been accumulated in favour of the non-elementary nature of a great number of substances then classed as elements.

Then again we can pass to another chemical philosopher, Kopp. In his researches on specific heats he also gives evidence to show that that relationship is not to be depended upon to establish the received view. If, then, the three greatest English chemists of their time we can name, and the most eminent chemical philosophers in France and Germany, give their opinion in behalf of the compound nature of the chemical elements, can these simpler forms be any other than those we detect by means of the spectroscope? By the conditions of the problem and the absence of knowledge they are not decomposable in the laboratory; if they were they would cease to be elementary bodies at once, and would be wiped out of our tables. Nor do I think it possible that in the present stage of our knowledge they can be revealed to us in any other way than by the spectroscope. It is unfortunate that none of these chemists who have given us this view have helped us by showing in what way the possibility, which all of them suggest, and which many of them intensely believe in, could be absolutely demonstrated; but it is obvious that if dissociation is the thing which time out of mind has made compound bodies simpler, in their minds the condition of higher temperature must have been present. The only difficulty was the way in which the effects of that high temperature could be measured and weighed, and I think that if the spectroscope had been introduced earlier they would probably have left some hints behind them which would have been of the greatest value to those who work with that instrument.

Passing from the chemists to the physicists, there is one, at all events, who has appreciated exactly how this decomposability of the terrestrial elements could be established. I refer to the lamented Clerk Maxwell. In his article on Atoms in the *Encyclopædia Britannica,* he says: "The discovery of a particular line in a celestial spectrum which does not coincide with any line in a terrestrial spectrum indicates either that a substance exists in the heavenly body not yet detected by chemistry on the earth, or" (and it is to the "or" I wish to draw attention) "that the temperature of the heavenly body is such that some substance undecomposable by our methods is there split up into components unknown to us in their separate states." Absolutely nothing could be clearer than this.

But in endeavouring to discuss the question as to how far the hypothesis of evolution of chemical forms is contrary to, or in accordance with the views of modern chemistry, we must draw a very wide distinction between chemical theory and chemical fact.

When we compare the laws given in average chemical text-books with the laws which lie at the root, let us say, of astronomy, the candid mind cannot fail to be struck by the difficulty which chemists must have encountered in endeavouring to reduce the face of their science to order, on the hypothesis they bring before us. An outsider thinks, for instance, that the basis of chemistry, or a large part of the basis of chemistry at all events, lies in the fact that the chemist has determined the existence of a certain number of elementary bodies, each of these elementary bodies having a certain atomic weight, and that this atomic weight determines all the constants of that body. Yet we read in chemical text-books that this atomic weight is fixed according to no invariable rule; indeed, with Kepler's laws and Newton's laws in the mind one comes to the conclusion that it is not too much to say that it is determined by a series of compromises. An outsider would think that if any one of these elementary bodies were taken as a standard, the weight of an equal volume of vapour of another substance under equal conditions would bear some relationship of a definite character to the atomic weight. This however is not the case. Again, among the questions to be considered as determining the atomic weights taken, is an assumed limitation of combination power, a so-called atomicity, according to which one substance is a monad, because it will combine with that same relative proportion of hydrogen which exists in half a water-molecule. Another substance is called a dyad, because it will combine with the same relative proportion of hydrogen which exists in a whole water-molecule, and so on. When we thus begin to class the substances into monads, dyads, hexads, and so forth—in fact, when we thus effect a re-classification of the elementary bodies, the solidarity at once breaks down; we find that the classification after all is useless, because the same substance may behave as a dyad, a tetrad, a hexad, a pseudo-tryad, and a pseudo-octad; in fact, one feels one is dealing with something that is more like a moral than a physical attribute—a sort of expression of free will on the part of the molecules. We are, I think, justified in asking whether these various attempts to formulate a science do not break down after a certain point, because they attempt to give a fixity to what is in truth variable.

When we pass to the *facts* of the science, the key-note of which is variability from one end of the scale to the other, we find that the view of successive dissociation, the view of variable molecular groupings brought about under different conditions, is really more or less in accordance with them, where the laws based on fixity break down entirely. Thus, for instance, let us take the question of vapour densities. The view accounts fully for the so-called anomalous vapour densities, and in this way: it suggests that the elements may really be complex groups which break up into their constituent groups under suitable conditions of temperature, as phosphoric chloride and many other bodies do when obtained in the condition of vapour. We have dissimilar groups in the one case, and possibly similar groups in the other. In this way, that contradiction in terms, the "monatomic molecule," really becomes the evidence of a higher law.

Let us pass to allotropic conditions. The explanation of these is that there are bodies which have a large molecular range within the ordinary temperatures at our command. The substances in which allotropism is most marked are all metalloids which have not been found in the sun, and the allotropic forms give us in many cases different spectra—spectra indicating a considerable complexity of the molecules which produce them about which something has been said already. In the passage from one allotropic condition to the other, energy, without any known exception, is absorbed or given out. What is it doing? if it is not in some way or other controlling the passage from one molecular group to another? These allotropic conditions, occurring very obviously to us in certain limits at our ordinary temperature and pressure are, possibly, but special cases of group-condensation common to all bodies, represented by Dalton's law of multiple proportions. We can indeed imagine a condition of things in which the difference between iron in the ferrous and ferric chlorides ($FeCl_2$ and Fe_2Cl_6) would be as obvious as the difference between ordinary and amorphous phosphorus.

In certain classes of so-called organic substances this grouping of simpler groups to more complex actually takes place, and is recognised under the term polymerism—for instance, with cyanogen compounds of oxygen we have a simple thing like cyanic acid (CNO) say, which will form a series of compounds, and we have its so-called polymers, $C_2N_2O_2$, or $C_3N_3O_3$, which will each form a series of compounds, these groups of more complex nature forming, by their combination, group-individuals with related but not identical properties with the simplest or fundamental group.

In many cases the amount of this condensation may be determined by the vapour densities. In others, again, a dissociation takes place at a certain limit of temperature, a simpler or fundamental group being the resolution product.

The resemblance between these cases of polymerism and especially those elementary bodies which exhibit allotropism, is at least striking.

In the one case, the organic complex bodies, the range of existence is in most cases within our easy attainment; in the so-called elementary stuffs it is less frequently the case. We can certainly convert ordinary phosphorus and sulphur into allotropic and most likely polymeric forms, but we do not know as yet how many atoms more are contained in the polymeric forms of these substances than in their simpler states.

And in other substances this range of condition of formation passes gradually out of our reach, but the phenomena are the same in kind up to the temperature of the sun. And again, when we can obtain the spectra of bodies like amorphous phosphorus we can prophesy that the relative grouping of the atoms of phosphorus in this to the ordinary form will be exhibited.

This brings us to the next point—atomicity. What are the associated phenomena? Lowest melting-point, simplest spectrum, lowest atomicity. Therefore we are justified, I think, in assuming that atomicity may after all be but the measure of the molecular groupings at work. In this way we can associate various atomicities, not with moral phenomena as regards the *behaviour* of the same molecule, but with different physical states—different complexities of the same substance. Thus in the same substance the more complex or allotropic the molecular grouping, the higher the atomicity. Hence the substances in which the highest atomicities appear should, as a rule, be formed and broken up at the lowest temperature. This, I am informed, is really what happens in the majority of cases.

I have ventured in these few remarks to touch upon the relations of the new view to modern chemical facts, because I think such a discussion shows us that there are several chemical regions in which the views can be tested from a chemical point of view, although I have, of set purpose dealt with them absolutely from the physical side. One such step of the highest interest has already been taken by Captain Abney. The language of Professor Roscoe, the President of the Chemical Society, in describing it, is so clear, and so admirably put, that it is impossible for any one to improve upon it.[1] Referring to the work which Captain Abney and Colonel Festing have done together, he says: This work "is no less than a distinct physical test of the existence in organic compounds, of the organic radicals, and a means of recognising the chemical structure of an organic compound by means of the spectroscope." This result "is accomplished by photographing the absorption spectra of organic compounds in the infra-red part of the spectrum. In these invisible portions characteristic and distinct absorption lines and bands occur for each organic radical. The ethyl compounds all show one special ethyl band; the methyl compounds a special methyl band; and thus, just as a glance at the luminous portion of the spectrum satisfies us of the presence of calcium, lithium, and rhubidium, so a simple inspection of these infra-red photographs enables us to ascertain the presence of the various organic radicals. This invention is still in its infancy, but one of greater importance to chemists has seldom if ever been communicated to the Society." I have been the more anxious to give these results in Professor Roscoe's own words, because it will be seen that, *mutatis mutandis*, these remarks touching the spectra of organic radicals are precisely identical with one statement I have been endeavouring to make with regard to inorganic radicals. It cannot therefore be said that the nature of the principle I bring forward is one with which chemists are not familiar.

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[1] *Journal of the Chemical Society*, May, 1881.

CHAPTER XX.

SOME TEST EXPERIMENTS.—ABSORPTION PHENOMENA.

We are now in a position to test the new hypothesis by a detailed reference to those solar phenomena and connected laboratory experiments which can bear upon it. But on the *festina lente* principle, which I have so far adopted, I propose in the first instance to state the results of several new modes of experimental investigation which I tried with the view of throwing light if possible upon the question of molecular complexity raised in 1873.

I have shown in chap. xiv. when dealing with the difficulties connected with the old view, the growing importance of the question of multiple spectra, and how, as the work went on, the number of observers who ascribed them to impurities got less and less. Some of the new methods have to do with the question of multiple spectra, and a brief reference to them will show that their results are almost entirely in favour of the new view, and that the progress of spectrum analysis increases the possible number of molecular groupings. We began with line spectra and fluted spectra as the maximum of variation; we end with line and fluted spectra both of complex origin, and new orders of spectra altogether, determined by absorption or radiation of a continuous sort in one or other part of the spectrum.

These points may be discussed seriatim, and I propose to take the last first.

When it first began to be conceded that the fluted spectrum of a substance was not due to an impurity, but to an allotropic state, it was thought that it was produced by molecules intermediate in complexity between those which in the case of any one body produced the continuous and line spectrum respectively. Some experiments which I undertook soon led me to the conclusion that this molecular grouping was not the only one between the extremes named. It was an inquiry which carried us into lower temperatures than had been employed before; it was less wonderful therefore that it should show that more "orders of spectra," to use Plücker's term, were necessary.

These experiments were undertaken in consequence of the following considerations:—

I. Most solids when heated—if they can be heated and yet retain their solid state—give us continuous radiation spectra.

II. Many of the metalloids in vapour give us fluted absorption spectra at ordinary temperatures, and others do the same at higher temperatures.

III. Most elements driven into vapour by the temperature of the voltaic arc give us line radiation spectra, with line absorption.

IV. All elements driven into vapour by the induced current give us line radiation spectra.

Now we have here, *prima facie*, good reason for supposing that the molecular structure of the vapours which give us such different effects is not the same. To take the lowest ground. If, in the absence of all knowledge on the subject, it could be shown that all vapours at all stages of temperature gave us spectra absolutely similar in character, then it would be more likely that all vapours were truly homogeneous and similar among themselves, as regards molecular structure, than if the spectra varied in character, not only from element to element, but from one temperature to another in the vapour of the same element. Further, the continuous spectrum of the solid element is observed in the case of some well-known compounds, whereas all known compounds are resolved by the high tension spark into their constituents. We have a right to assume, therefore, that an element in the solid state is a more complex mass than the element in a state of vapour, since its spectrum is the same as that of a mass known to be more complex. Again, when changes occur in the spectrum of the same substance, they are always in the same direction; and, further, the spectra we obtain from elements in a state of vapour are similar in character to those we obtain from vapours of known compounds.

So far we have continuous, fluted, and line spectra, but this is not all; certain bodies, both simple and compound, when we study them by their absorption at ordinary temperatures, give us phenomena not included in the above statement.

Here is what I said on this subject in 1874[1]:—

"At ordinary temperatures in some cases, as in selenium, the more refrangible end is absorbed: in others the continuous spectrum in the blue is accompanied by a continuous spectrum in the red. On the application of heat, the spectrum in the red disappears, that in the blue remains; and, further, as Faraday has shown in his researches on gold leaf, the masses which absorb in the blue may be isolated from those which absorb in the red. It is well known that many substances known to be compounds, in solution give us an absorption in the blue or blue and red; and also that the addition of a substance known to be a compound (such as water) to substances known to be compounds which absorb the blue, superadds an absorption in the red.

"In those cases which do not conform to what has been stated, the limited range of the visible spectrum must be borne in mind. Thus I have little doubt that the simple gases at the ordinary conditions of temperature and pressure have an absorption in the ultra-violet, and that highly compound vapours and liquids are often colourless because their absorption is beyond the red, with or without an absorption in the ultra-violet. Glass is a good case in point; others will suggest themselves as opposed to the opacity of the metals."

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[1] *Proc. R. S.* No. 153, 1874.

The point in the new experiments, then, to which reference has now to be made was to determine what spectrum the vapours of the metallic bodies gave at a lower temperature than any at which they had been previously observed. It is evidently therefore a question of absorption phenomena.

The first experiments were made, in December, 1873, upon zinc in a glass tube closed at each end with glass plates; they were made by myself in conjunction with my friend, Dr. Russell. We studied the absorption of the light of the positive pole of the electric lamp placed at one end of the tube by the vapour in the tube. A one-prism spectroscope was used for the observations. A stream of pure hydrogen was allowed to pass through the tube, which was heated in a Hoffmann's gas furnace. This plan, however, would not work for several reasons—among them, the glass melted.

In the next attempt we used an iron tube some four feet long, closed with glass at the ends as before, with its central part in a charcoal furnace. The air in the tube was replaced by hydrogen and the substance of which the low temperature absorption spectrum was to be examined was dropped, by a special arrangement, into the central portion. There was an electric lamp at one end of the tube and a spectroscope at the other, as in the first arrangement. The object of keeping the voltaic arc outside the tube was of course to enable us to deal with the vapour at the temperature of the furnace merely. In most cases, when the reduction of the density of the vapour enabled any light to be seen, the light was green, and the spectroscope indicated that this was due to the absorption of the red and blue light. Various experiments indicated that the blue and red absorptions were independent of each other, for they varied with temperature.

We found that the temperature reached by the tube could be marked by the following phenomena :—

I. When the continuous spectrum of the tube extended to D this line not being visible.

II. When it extended beyond D, D being bright.

III. When it extended into the green, D being very bright.

IV. When it extended beyond the green, D being invisible.

Before I detail some of the results of this inquiry I should state that Professor Roscoe, who had seen some of the earlier experiments, also took the matter up, and commanding a higher temperature than was at first at our disposal, observed some previously unrecorded spectra in the case of sodium and potassium. These I shall call the new spectra. They are always easily visible when a sufficient temperature has been reached.

The results obtained were as follows :—

Hydrogen	No absorption.
Nitrogen	
Mercury	
Bismuth	
Zinc.	Continuous absorption in the blue.
Cadmium.	
Antimony.	
Phosphorus.	
Sulphur.	Fluted spectrum (previously observed by Salet).
Iodine.	Fluted spectrum in the green, and intense band of general absorption in the violet where, at the ordinary temperature, the vapour transmits light.
Sodium.	There were observed either separately or together— α. D absorbed. β. Continuous absorption breaking up in the middle into red and blue absorption. γ. The new spectrum.

Potassium. There were observed either separately or together—
- α. Line absorption more refrangible than D.
- β. As in Sodium.
- γ. The new spectrum.

An expansion of this research by Professor Roberts and myself added great weight to the views expressed above and considerably increased the number of bodies possessing fluted spectra.

The experimental conditions were varied as follows:—The temperature was greatly increased by the employment of an oxyhydrogen blowpipe and a lime still, such as that employed by Stas, but modified so that the metallic vapour might be conducted into a lime-tube or funnel heated to redness, and so placed that a beam from an electric lamp would readily traverse it.[1]

The general results of this investigation may be stated as follows:—

New fluted spectra.

Silver.
Manganese.
Chromium.
Bismuth.
Selenium.

Absorption in the blue.

Silver.
Manganese (red also).
Copper.
Aluminium.
Cadmium (red also).
Iron.
Cobalt.
Nickel.

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[1] Full details of the arrangements will be found in *Proc. R. S.* No. 160, 1875, and also in *Studies in Spectrum Analysis.*

Absorption in the blue—(continued.)

Tin (red also).
Lead (red also).
Antimony.
Gold (red also).
Palladium.

A discussion of these observations at the temperature of the oxyhydrogen flame with those made at the lower temperatures seemed to prove beyond all question that the molecules which absorbed the blue were broken up to produce the one with the fluted spectrum, and similar reasoning indicated that the blue molecules were produced at the expense of the red ones.

The reasoning was the same as that which had been held to demonstrate the different complexities of the molecules which gave line and fluted spectra respectively. Unfortunately we were in both cases almost beyond chemical and other physical inquiry —almost, but not quite; and where other inquiry was possible, it gave no uncertain sound. It is important that these other inquiries should be referred to in some detail.

In the case of gold it is possible to separate the blue and red molecules, which I contend produce the colour of gold leaf, and to get them into different bottles—that *ne plus ultra* of chemical separation. The beautiful researches of Faraday on this substance led him to the conclusion that we were by the colour phenomenon brought really into presence of a different molecular structure, but he worked in the prespectroscopic days, and each different colour to him represented a molecule. Hence he held that the molecules existed "of intermediate sizes or proportions."[1] I hold, however, that a twofold complexity is sufficient to explain all the phenomena. We have absorption of the blue giving us red gold, absorption of the red giving us blue gold, and absorption both of blue and red giving us green gold.

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[1] *Researches in Chemistry*, p. 417.

Again, it is known that by compression we can turn green chlorine vapour—green because it absorbs the blue—into yellow liquid chlorine—yellow because an absorption in the red is added; and the changes in colour of sulphur and phosphorus are also in accordance with the hypothesis. There is also another proof behind of a novel character, to which a slight reference has already been made on p. 211. In my work on spectrum photography I had been struck with the possible connection between the continuous absorption in the blue recorded in the above experiments, and the continuous action in the blue of the salts of silver, and it seemed probable that further study would provide us with continuous action in the red, if we could either complicate the molecular structure of the salt of silver or use some other metal altogether. I invited my friend Dr. Russell to join me in this research, but other work made it impossible. Still, however, we had not long to wait, and I have already recorded with what success Captain Abney has worked in this direction, having discovered a method of obtaining the silver salt of the same *chemical* nature but more complex in its *molecular* arrangement.

It may be argued that because we observed more absorption in the red with the oxyhydrogen flame than we did in the cooler tube, that the red absorption may have been produced by the breaking up of the blue-absorbing molecule. But this is not so, because the blue was produced at last at the expense of the red, and the fact that the red-absorbing vapour is produced more richly at higher temperatures is in accordance with what we know of hydrocarbon vaporizing, if we assume it to be more complex. In organic work the higher the temperature used to break up a compound the richer in complex forms is the vapour first given off.

So much then for the independent confirmation of the existence of red and blue molecules, and of the fact that the one which absorbs the red is more complex than the one which absorbs the blue light.

Have we the like information in the case of the passage from the blue molecule to the fluted one? I think we have in the case of sulphur; and here we approach a question of great interest to us in our inquiries—that of vapour densities, on which something must be said before the question of sulphur is discussed.

Chemists, by their vapour-density determinations, endeavour, so far as the elements are concerned, to ascertain the weight of say a cubic inch or a litre of any permanent gaseous element or of the vapour of any liquid or solid element; and in order that relative statements may be made they take the weight of one of the permanent gases, hydrogen, as the unit, because they have found that this is the lightest. Thus, in the case of the permanent gases hydrogen, oxygen, and nitrogen, we may construct the following table, showing also the atomic weights, hydrogen being taken as the unit in that case also—

	Vapour density.	Atomic weight.
Hydrogen	1	1
Nitrogen	14	14
Oxygen	16	16

In addition to the permanent gases, there are three other bodies which readily lend themselves to such determinations; these are chlorine, bromine, and iodine. Tabulating out the results, we get—

	Vapour density.	Atomic weight.
Chlorine	35·5	35·5
Bromine	80	80
Iodine	127	127

As before, we find the vapour density on all fours with the atomic weight, and represented by the same values. Hence with chemists the "normal" state is that the vapour density and atomic weight as referred to hydrogen shall be represented by the same values.

Unfortunately, however, in the case of most of the elementary bodies which exist generally in the solid state, the determination of vapour densities is either very difficult or impossible, so that in addition to those we have named the facts have only been ascertained in the case of cadmium, mercury, phosphorus, arsenic, and sulphur, and all these are "anomalous," that is, a "law" founded on six cases fails in the next five tried.

In the first four of these we have two distinctly opposed results—

	Vapour density.	Atomic weight.
Cadmium	56	112
Mercury	100	200
Phosphorus	62	31
Arsenic	150	75

That is to say, we have cases in which the density is half, others in which it is double, the atomic weight.

To bring out the facts in the preceding table, a very concrete illustration may be pardoned. Suppose the complexity of the bodies in the solid state to be represented by something higher than a penny, the pennies being changed into halfpennies, or even into farthings, by various dissociating agencies. Next, let us assume that the molecular fineness produced by chemical action and represented by the weighed atom in each case is represented by a halfpenny. In the case of cadmium and mercury the temperature needed to produce the vapour has done more than chemical action, we get farthings. In the case of phosphorus and arsenic temperature does less, and we get pennies. Now as a matter of fact cadmium and mercury are sluggish chemically, while phosphorus and arsenic are active.

The interesting point about these determinations is that where there is chemical confusion there is also spectroscopic confusion. If we can attach certain spectroscopic characteristics to certain molecular finenesses (as many chemists were prepared to do before they found out where it would lead them), then when we find ourselves in presence of the true chemical molecule in each case we should expect to find similar spectra. Thus, to give an instance, it was at first suggested that, since "atoms" were supposed to give line spectra and "molecules" fluted spectra, cadmium and mercury vapour, the vapour densities of which lead chemists to believe that they are dealing with the "atoms" of these substances, should give us line spectra. This, however, they do not do.

The spectroscopic confusion will be gathered from the following table :—

Hydrogen	No absorption.
Nitrogen	No absorption.
Oxygen	Flutings.
Chlorine	Absorption in the blue, and flutings.
Bromine	Flutings.
Iodine	Flutings, with or without continuous absorption in the violet according to temperature.
Cadmium	Absorption in the blue.
Mercury	No absorption.
Phosphorus	Probable flutings.
Arsenic	Probable flutings.

Evidently therefore, so far as spectroscopic evidence is concerned, these vapours do not exist in comparable molecular conditions, and this at once explains the "anomalies."

We are now in a position to consider the case of sulphur. This body, which is well within the range of experiment, gives us two vapour densities, which we can tabulate as follows :—

	Vapour density.	Atomic weight.
Sulphur—		
Above 1000°	32	32
Below 1000°	96	

Now to the receptive mind there is nothing "anomalous" about this: a higher temperature brings about simplification, *violà tout!* And what says the spectroscope? On passing from the density of 96 to that of 32, the vapour changes its spectrum from one giving continuous absorption in the blue to one of flutings.

Here then we have, if vapour density determinations be worth anything, an undoubted change of molecular grouping accompanied by a spectroscopic change exactly in the direction required by other considerations.

Have we then similar evidence having regard to the change from the fluted spectrum to the line spectrum? We have, and it is precisely of the same nature as that afforded by sulphur, except that the temperature employed is higher.

For many years the sulphur result stood alone, but abundant evidence has now been brought forward to show that iodine and bromine behave exactly in the same way, that is they have two different vapour densities at two different temperatures.

	Vapour density.	Atomic weight.
Bromine—		
Above 2000°	40	80
Below 2000°	80	
Iodine—		
Above 2000°	63·5	127
Below 2000°	127	

This change of vapour density occurs at the temperature at which the fluted spectra give place to line spectra.

The absence of sodium and potassium from the above lists may have been remarked, since they vaporize at low temperatures. Very great difficulties have presented themselves in experiments on these bodies, and various values have been recorded of the density of their vapours. It is the opinion, however, of many, that it changes perhaps twice with the temperature as one would expect, and we have already referred to the series of spectra given by these bodies at comparatively low temperatures, from which we should have predicted these changes of vapour density.

To sum up, then, this concurrent testimony, we do find evidence, other than spectroscopic, of the existence of molecular groupings of the elementary bodies in different degrees of fineness; and if we endeavour to explain the "anomalous" vapour density of some of the substances to which attention has been drawn by continuing the same line of argument, it is easy to see that it can be easily and simply explained in this way. If the molecule present in the vapour at the time it is weighed is that same molecule to which the mass of the element is most broken down by chemical affinity, the density will be normal. If the molecule is finer, we shall have the condition of things represented by cadmium and mercury; if coarser, the state of things represented by phosphorus and arsenic. It is further interesting to note that, of the permanent gases, the one most easily driven into the line stage, hydrogen, is a monad; while sodium, lithium, potassium, chlorine, bromine, and iodine, bodies most easily driven into vapour, are also monads.

I wish here to give in confirmation of my own work a reference to the work of others on the possible complex origin of line spectra. One of the most exhaustive inquiries of this nature was published by Dr. Schuster in the *Philosophical Transactions* in 1879. The subject was Oxygen. He distinguishes no less than four spectra of this substance, two of them being true line spectra. In order to make the reference complete I give his description of all four.

I. *The Elementary Line-Spectrum.*—This is the spectrum which appears at the highest temperature to which we can subject oxygen. That is, whenever the jar and air-break are introduced into the electric circuit. It consists of a great number of lines, especially in the more refrangible part of the spectrum. It has been called elementary line-spectrum to distinguish it from the other line-spectrum, because, according to one hypothesis which has been suggested to explain the variability of spectra, the molecule which gives this spectrum is in a simpler or more elementary state than that which gives the other so-called compound line-spectrum. We may, however, adopt the nomenclature independently of any hypothesis that may have suggested it.

II. *The Compound Line-Spectrum.*—This spectrum appears at lower temperatures than the first. It consists of four lines: one in the red, two in the green, and one in the blue. With the exception of the blue line all the lines in this spectrum widen very easily, and, with an increase of pressure, more easily even than the hydrogen lines. They do not widen out equally on both sides, but more towards the red than towards the violet. This fact is especially noticeable in the more refrangible of the two green lines. The blue always remains sharp.

III. *The Continuous Spectrum of Oxygen.*—This spectrum appears at the lowest temperature at which oxygen is luminous. The wide part of a Plücker tube, filled with pure oxygen, generally shines with a faint yellow light, which gives a continuous spectrum. Even at atmospheric pressure this continuous spectrum can be obtained by putting the contact-breaker of the induction coil out of adjustment, so that the spark is weakened. According to Becquerel an excess of oxygen in the oxyhydrogen flame produces a yellow colour, which is very likely due to this continuous spectrum. The continuous background which often accompanies the elementary line-spectrum must not be confounded with this spectrum.

IV. *The Spectrum of the Negative Glow.*—This spectrum, which was first accurately described by Wüllner, is always seen in the glow surrounding the negative electrode in oxygen. It consists of five bands: three in the red and two in the green. The least refrangible of the red bands is so weak that it easily escapes observation; the two other red bands are rather near together, and may be taken for a single band if the dispersion applied is small. The two green bands, which appear of the same brightness throughout, with pretty sharply-defined edges, are resolved into a series of lines when looked at with high optical powers.

Dr. Schuster gives the following description of the appearance of a vacuum-tube filled with pure oxygen, as it undergoes gradual exhaustion, in order to give an idea of the way in which the spectra of oxygen gradually diffuse into each other :—

> At first the spark has a yellow colour, and the spectrum is perfectly continuous. Almost immediately, however, four lines are seen in the capillary part above the continuous spectrum. One of these lines is in the red, two in the green, and one in the blue. The discharge still passes as a narrow spark throughout the length of the tube. In the wide part the spectrum remains continuous, and it extends more towards the red and blue than in the capillary part. It seems as if the four lines had taken away part of the energy of the continuous spectrum. As the pressure diminishes these lines increase considerably in strength, the spark spreads out in the wide part of the tube, and the intensity of the continuous spectrum is therefore considerably diminished, while it still forms a prominent part in the spectrum of the capillary part. When the pressure is small the continuous spectrum decreases in intensity. At the same time the negative glow, with its own characteristic spectrum, gradually extends through the negative half of the tube into the capillary part. The continuous spectrum has now entirely disappeared, and the bands of the negative pole and the four lines stand out on a perfectly black background. It is under these conditions that the change from the compound line-spectrum to the elementary line-spectrum is best studied. The mere insertion of a Leyden jar makes hardly any difference; the jar does not seem to be charged at all. If, in addition to the jar, we insert a moveable air-brake which can be opened or closed at will, while we look through the spectroscope, we shall be able to see alternately, two perfectly distinct spectra. If the air-brake is closed, the four lines of the compound-spectra only are seen, if the air-brake is opened, these four lines will disappear entirely, and the elementary line-spectrum will come out. We have here as complete a transformation as we have from the band to the line-spectrum of nitrogen, taking place under exactly the same circumstances.[1]

The following extract from Dr. Schuster's paper will show that he accepts the view of molecular simplification that I advocate:—

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[1] *Phil. Trans.* clxx. 51.

"According to [one] view, liquid and solid bodies give generally continuous spectra, not because they are liquid or solid, but because in these states the molecules have a more complicated structure than in the gaseous state. Experiment has to decide between the two theories, the theory of molecular disturbance and the theory of molecular structure. I think the facts are decidedly in favour of the latter theory. Mr. Lockyer's investigations have shown that most bodies give us a continuous spectrum, as a gas, before they condense, and many at a considerable temperature above the boiling point. Mr. Lockyer has rightly drawn the conclusion from this fact, that the atomic aggregation of the molecules is the cause of the different orders of spectrum. If we observe the changes in a spectrum which gradually take place on heating or cooling a vapour, we find that the continuous spectrum is produced, not by a widening of the bands, but by a direct replacement, which is sometimes sudden and sometimes gradual, and which leaves no doubt in the observer's mind that he has to deal with two vibrating systems, and not simply with a disturbed one; I do not, of course, mean to say that the impacts of other molecules have no observable influence. If the hydrogen lines widen through increased pressure, it is very likely that the alteration is produced by impacts; but the change from a line-spectrum to a continuous-spectrum, as a rule, is quite different from the change which takes place with hydrogen. According to the theory of molecular aggregation, it seems quite possible that a liquid should give the same spectrum as its vapour, and this indeed seems to be true in some cases."[1]

In a subsequent note Dr. Schuster adds—

"That the discontinuous spectra of different orders (line and band spectra) are due to different molecular combinations I consider to be pretty well established; and analogy has led me (and Mr. Lockyer before me) to explain the continuous spectra by the same cause; for the change of the continuous spectrum to the line or band spectrum takes place in exactly the same way as the change of spectra of different orders into each other."[2]

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[1] *Phil. Trans.* Part I. 1879, p. 38.

[2] *Loc. cit.* p. 3.

This chapter may fitly end by a quotation from Professor Berthelot, who has been studying elementary chemical substances from quite another standpoint.

"L'étude approfondie des propriétés physiques et chemiques des masses élémentaires, qui constituent nos corps simples actuels, tend chaque jour davantage à les assimiler, non à des atomes indivisibles, homogènes et susceptibles d'éprouver seulement des mouvements d'ensemble,—il est difficile d'imaginer un mot et une notion plus contraires à l'observation; mais à des édifices fort complexes, doués d'une architecture spécifique et animés des mouvements intestins très variés."—Berthelot, *Comptes Rendus*, 1880, vol. 90, p. 1512.

CHAPTER XXI.

SOME TEST EXPERIMENTS.—TRIAL OF NEW METHODS.

THERE was still however a difficulty in many minds in ascribing changes in spectra to temperature alone, and therefore to a possible dissociation. It thus appeared to be one's duty to find an *experimentum crucis* if possible.

It seemed as if the results of experiments based on the following considerations ought to be accepted as throwing light on the question.

First Consideration.

At a low temperature some substances give us few lines while at a high one they give us many. Vapours, therefore, already glowing with few lines at a low temperature, say in a flame, should give us all their lines when the vapour is suddenly subjected to a high one, say by the passage of a high tension spark. On the bell hypothesis the spectrum should change with the mode of striking. On the dissociation hypothesis this should only happen for the lines of those molecular groupings which are *from other considerations* held to be more simple. If the flame has brought the substance to its lowest state, the passage of the most powerful spark should not cause the flame spectrum to vary.

Now what are the "other considerations" above referred to? This necessitates a slight digression.

Of the short lines (see *ante*, p. 213), the explanation generally given and accepted was that they were produced by a more complex vibration imparted to the "atom" in the region of greatest electrical excitement, and that these vibrations were obliterated, or prevented from arising, by cooling or by admixture with dissimilar "atoms."

Subsequent work, however, has shown[1] that the different behaviour of these lines seemed to suggest the probability *that not all of the short lines of spectra were, in reality, true products of high temperature.*

Now if not all but only *some* of the short lines are products of high temperature, we are bound to think that the *others* are remnants of the spectra of those molecular groupings first to disappear on the application of heat.

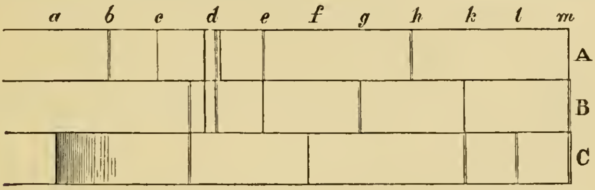

FIG. 97.—A. Highest temperature. C. Lowest temperature.

At any particular heat-level, then, some of the short lines may be due to the vibrations of molecular groupings produced with difficulty by the temperature employed, while others may represent the fading out of the vibrations of other molecular groupings produced on the first application of the heat.

In the line of reasoning advanced in Chap. 18,[2] both these results are anticipated, and are easily explained. Slightly varying Fig. 86, we may imagine furnace A to represent the temperature of the jar spark, B that of the Bunsen burner, and C a temperature lower than that of the Bunsen burner (Fig. 97).

Then in the light of the hypothesis the lines *b* and *c* if seen in the spectrum of B would be truly produced by the action of high temperature, while *a*, *f*, and *l* would be but remnants of the spectrum of a lower temperature.

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[1] *Proc. R. Soc.* vol. xxviii. p. 159.

[2] *Ibid.* p. 162.

So much then by way of explanation; it is clear that to make this reasoning valid we must show that the spark, or better still the arc, provides us with a summation of the spectra of various molecular groupings into which *the solid metal which we use as poles* is successively broken up by the action of heat.

We are not limited to solid metals; we may use their salts. In this case it has already been shown[1] that in very many cases the spectrum is one much less rich in lines.

Second Consideration.

Since at different temperatures the brilliancy of the spectral lines of the same substances as ordinarily observed changes enormously, it is important to see if these changes can be produced *at the same temperature* by employing those experimental conditions which will be most likely to bring about different molecular conditions if such exist.

This experiment is founded on the behaviour of compound bodies when they are distilled at different temperatures. If we take, for instance, a mixture of hydrocarbons, some of them very complex in their nature, and others more simple; when a low temperature is employed it is found that the simpler hydrocarbons come over in the shape of vapour in greater abundance, when a higher temperature is employed more complex forms come over in greater abundance. If therefore we were fortunate enough to be able to observe the spectra of these different vapours, assume that the series of hydrocarbons, for instance, shown in the accompanying diagram (Fig. 98), had each of them a distinct spectrum, we should be able to follow spectroscopically the effect of each change of temperature, and we could in that way associate the known fact of the greater abundance of dense vapour which comes over at a higher temperature with a spectrum of a certain kind.

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[1] *Phil. Trans.* 1873, p. 258.

Now in our experiment we deal not with a compound body in the ordinary sense, but with a so-called elementary body. The question is, Will this make any difference; or, rather, shall we get similar differences?

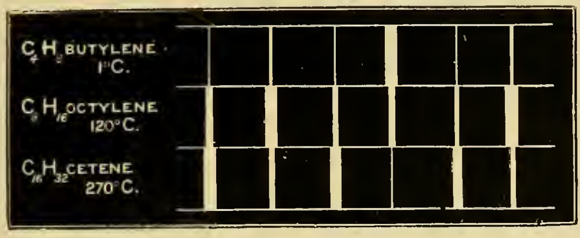

Fig. 98.—Hypothetical spectra obtained on distilling a successively increasing temperatures a mixture of light and heavy hydrocarbons.

The experimental work has followed two distinct lines. I shall refer in detail to the results obtained along each.

Experiments based on the First Consideration.

The first method of investigation adopted consists in volatilising those substances which give us flame spectra in a Bunsen flame and passing a strong spark through the flame, first during the process of volatilisation, and then after the temperature of the flame has produced all the simplification it is capable of producing.

The results have been very striking; the puzzles which a comparison of flame spectra and the Fraunhofer lines has set us find, I think, a solution; while the genesis of spectra is made much more clear.[1]

To take an instance, the flame spectrum of sodium gives us, as its brightest, a yellow line, which is also of marked importance in the solar spectrum. The flame spectra of lithium and potassium give us, as their brightest, lines in the red which have not any representatives among the Fraunhofer lines, although other lines seen with higher temperatures are present.

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[1] I allude more especially to the production of triplets, their change into quartets, and in all probability into flutings, and to the vanishing of flutings into lines, by increasing the rate of dissociation.

Whence arises this marked difference of behaviour? From the similarity of the flame spectrum to that of the sun in one case, and from the dissimilarity in the other, we may imagine that in the former case—that of sodium—we are dealing with a body easily broken up, while lithium and potassium are more resistant; in other words, in the case of sodium, and dealing only with lines recognised generally as sodium lines, the flame has done the work of dissociation as completely as the sun itself. It is easy to test this point by the method now under consideration, for if this reasoning be correct (1) the chief lines and flutings of sodium should be seen in the flame itself, and (2) the spark should pass through the vapour after complete volatilisation has been effected without any visible effect.

Observation and experiment have largely confirmed these predictions. Using two prisms of 60° and a high-power eyepiece to enfeeble the continuous spectrum *of the densest vapour produced at a high temperature*, the green lines, the flutings recorded by Roscoe and Schuster, and another coarser system of flutings, so far as I know not yet described, are beautifully seen. I say largely, and not completely, because the double red line and the lines in the blue have not yet been seen in the flame, either with one, two, or four prisms of 60°, though the lines are seen *during volatilisation* if a spark be passed through the flame. Subsequent inquiry may perhaps show that this is due to the sharp boundary of the heated region, and to the fact that the lines in question represent the vibrations of molecular groupings more complex than those which give us the yellow and green lines. The visibility of the green lines, *which are short*, in the flame, taken in connection with the fact that they have been seen alone in a vacuum tube, is enough for my present purpose.

With regard to the second point; the passage from the heat-level of the flame to that of the spark after volatilisation is complete produces no visible effect, indicating that in all probability the effects heretofore ascribed to *quantity* have been due to the presence of the molecular groupings of greater complexity. *The more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen.*

Experiments based on the Second Consideration.

The second experimental method deals with vapours of elementary bodies volatilised at different temperatures in vacuum tubes. Many of the phenomena are quite new, and lines thus seen alone and of surpassing brilliancy, are those seen as short and faint in ordinary methods of observation.

The novelty of the method consists in the use of the luminous electric current as an explorer and not as an agent for the supply of the vapours under examination; that is to say, the vapours are first produced by an external source of heat, and are then rendered luminous by the passage of the current. The length and bore of the tube therefore control the phenomena to a certain extent.

A form of apparatus which I have found to answer very well is shown in the accompanying woodcut (Fig. 99).

A is the tube or retort containing the metal experimented on in its lower extremity, and having a platinum wire sealed into it at a distance of about two inches from the lower end, the other end being drawn out and connected by a mercury joint to an ordinary Geissler tube, which is connected by another mercury joint to the Sprengel pump C.

Another form of tube which I have used is prepared by inserting two platinum poles into a piece of combustion tubing sealed at one end, and after inserting the metal to be experimented on, drawing out the glass between the platinums to a capillary tube.

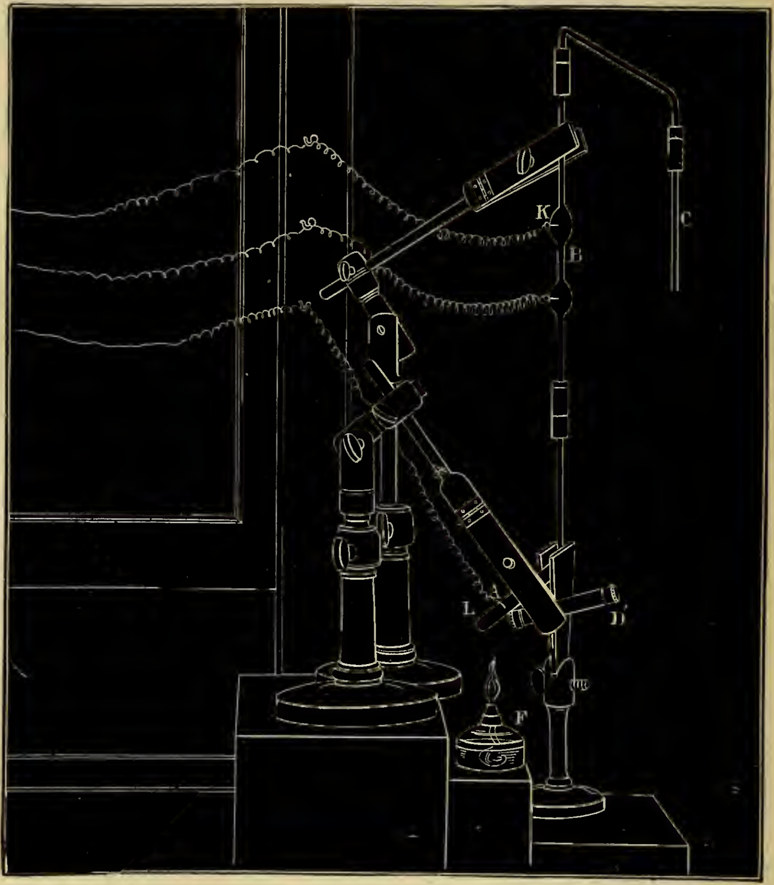

Fig. 99.—1st Form. Position of spectroscope when observing vapours close to the metal.

I have also tried inserting the platinum pole at the end of the retort, so that the spark passes from the surface of the metal, but this arrangement did not answer at all.

Some other modifications have been tried, but the first form I have described is that which I have found to answer best, so far as the trials have yet gone.

A is the tube containing the metal;

B, the capillary;

C, the connection with the pump.

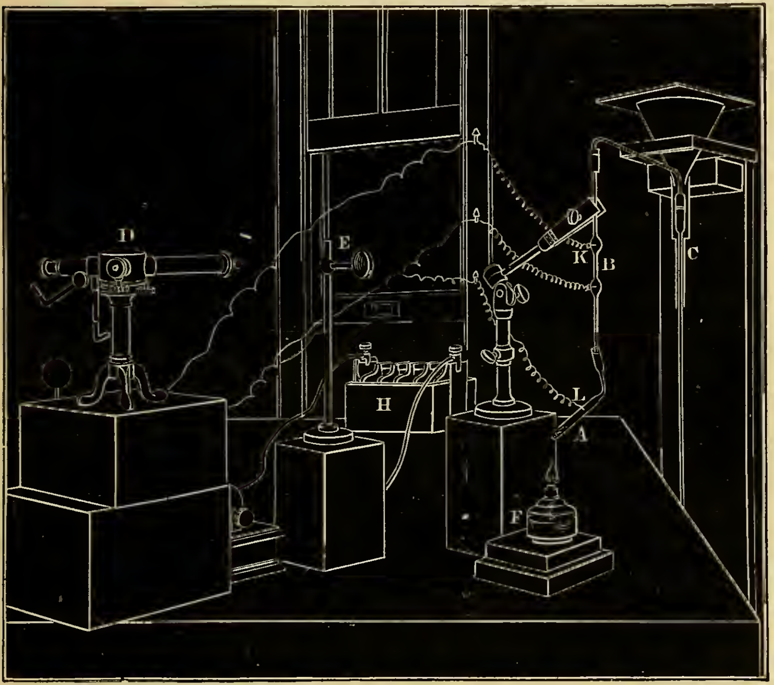

Fig. 100.—2nd Form. Spectroscope and lens in position for observing spectrum of capillary.

In both forms D is the spectroscope, a direct vision being used in the first case.

E is the lens used for focusing the image of the Geissler tube on the slit in the second case.

F is the spirit-lamp for heating the retort.

H is the battery.

K and L are the wires connected with the coil.

In the second form (Fig. 100) the method of observing the spectrum of the vapours close to the surface of the metal is indicated.

For determining the exact positions of the lines in the spectrum of the vapour in any part of the retort, a larger spectroscope, with its illuminated scale, was used in the place of the direct-vision spectroscope.

The secondary wires of the coil were connected, one with the pole in the upper bulb at B, and the other with the platinum at A.

B is an ordinary Geissler tube with two bulbs separated by a capillary tube. The great advantage of this arrangement is that this capillary portion can be used for ascertaining what gases or vapours are carried over by the pump without any interference with the retort, both wires being connected with the Geissler tube. If, for example, we are working with sodium which contains an impurity of hydrocarbon, the moment at which it begins or ceases to come off can be found by examining the spectrum of this capillary tube.

I now give an account of the phenomena observed when we were working with sodium, in order to indicate the changes observed.

After a vacuum has been obtained the retort is heated gradually. The pump almost immediately stops clicking, and in a short time becomes nearly full of hydrogen. The spectrum of the capillary then shows the hydrogen lines intensely bright. After some time the gas comes off far less freely, and an approach to a vacuum is again obtained. Another phenomenon now begins to show itself: on passing the current a yellow glow is seen, which gradually fills the whole space between the pole in the retort and the metal; its spectrum consists of the lines of hydrogen and the yellow line of sodium, the red and green lines being both absent until the experiment has gone on for some time.

As the distillation goes on, the yellow glow increases in brilliancy, and extends to a greater distance above the pole, and the red and green lines presently make their appearance as very faint lines.

The upper boundary of the yellow is quite sharp, the lines and fluted spectrum of hydrogen appearing above it.

After the yellow glow-giving vapour (which does not attack the glass) has been visible for some time, the pump is stopped and the metal *heated more strongly.* On passing the current a little while afterwards, a very brilliant leaf-green vapour is seen underlying the yellow one, and connected with it by a sap-green vapour. The spectra then visible in the tube at the same time are—

Leaf-green ...Green and red lines of sodium and C of hydrogen; D absent.
Sap-green ...Green, red, and yellow sodium lines of equal brilliancy, and C of hydrogen.
Yellow ...D alone and C.
Bluish-green ...C and F and hydrogen structure.

To observe the green sodium line alone it is necessary to point the direct-vision spectroscope just above the surface of the metal where the green is strongest. It is also necessary to guard against internal reflections from the glass, as this may sometimes cause the D line to be seen by reflection from the surface.

This method of inquiry has been tried also with potassium, calcium, and some other metals, and with metallic salts.

With potassium and calcium we get the same inversion of phenomena; the yellow-green lines of potassium being seen without the red; while in the case of calcium the blue line alone was seen. We get, as before mentioned, vapours which at one and the same time exhibit different colours and different spectra at different levels in the tube.

It may be advantageous to dwell on the details observed in the case of potassium. The tube is very perfectly exhausted, and then gently warmed with a spirit-lamp, the exhaustion going on during the whole process. On passing a current between the platinum electrodes we see a beautiful green glow in the tube, and obtain a certain spectrum. On replacing the spirit-lamp by a Bunsen burner we find as the result of this increased temperature that the colour in the tube changes to blood-red, and the spectrum is entirely different. The spectrum of potassium is one which requires a very great deal of study, for the reason that it varies very much under different experimental conditions. If the potassium, as is well known, is thrown into a Bunsen burner, the chief line that one gets is a red one. Kirchhoff, in the early days of solar chemical investigation, pointed out that this red line is not to be found among the Fraunhofer lines. The flame also gives us a line in the blue. If we examine the spectrum of potassium by means of an induction-coil we find the blue line which we also see in the flame, but it is intensified in the spark. We also see some strong lines in the green and yellow, which are barely visible in the flame—which are in fact not generally recorded in the flame-spectrum of potassium, although they are really visible when considerable dispersion is employed. These lines in the yellow and green become prominent lines. Now, it so happens that some of these lines in the green do, it is believed, correspond with Fraunhofer lines, and we are, therefore, justified in assuming that they represent a something, whatever it may be, in the potassium, which can withstand the heat of the sun, while the red lines represent something which is broken up at the temperature of those regions of which we can determine the absorption. The interesting point of the experiment, therefore, is this: assuming for a moment that the red line does represent a complex something which cannot withstand the temperature of the sun, and that the yellow line represents a something finer which can withstand the temperature of the sun, what happens when we try to drive off the vapour of this potassium *at the lowest temperature* at which we can get it to volatilise at all, is that if the experiment is carefully performed it gives precisely those lines which are reversed in the solar spectrum alone; and of that line which is the strongest line at the temperature of the Bunsen burner we see absolutely nothing at all. Referring to the spectrum which we get in the lilac and yellow-green part of the tube, two out of the three lines visible at all events are seen in the sun, whereas the other lines which we get in the flame and some of them which we get with an induction coil are not represented in the fine vapour which was produced at the lowest possible temperature.

While heated with the Bunsen burner some very exquisite colour-effects are seen in the tube, and especially a beautiful blood-red colour which might be imagined to be the product of that molecule which gives the red line seen in the Bunsen flame; but that is not the fact. The line seen in the flame of the Bunsen burner is not visible as a rule in the vapour when heated in this way, the lines actually seen being more refrangible.

The experiment then comes to this. If we assume potassium to be a compound body and that its finer constituent molecules are those which resist the solar temperature, then it behaves exactly like a mixture of hydrocarbons is known to do, that is, the finer vapours come off in greatest quantity at the lowest temperature, and the more complex ones as the temperature is raised.

The result of the application of this new method indicates that in the case of a considerable number of chemical substances not only is the line spectrum compound in its origin, as I suggested many years ago, but that a large number of the lines is due to molecular groupings of considerable complexity, which can be kept out of the reaction by careful low temperature distillation. Not only do these new results indicate an almost undreamt-of variability in spectra, but so far as I can see they are simply and sufficiently explained on the assumption that the elementary bodies in the solid state consist of various sets of molecules which behave like mixtures of organic compounds.

CHAPTER XXII.

THE SOLAR ATMOSPHERE ON THE NEW HYPOTHESIS.

BEFORE we attempt to apply further tests we must thoroughly consider the change of front in solar research necessitated by the introduction of the view of the possible dissociation of elementary bodies at solar temperatures. We must first consider what solar facts we may expect on the two hypotheses to compare with the result of laboratory experiments.

On the old hypothesis the construction of the solar atmosphere was imaged as follows :—[1]

(1.) We have terrestrial elements in the sun's atmosphere.

(2.) They thin out in the order of vapour density, all being represented in the lower strata, since the temperature of the solar atmosphere at the lower levels is incompetent to dissociate them (see p. 168.)

(3.) In the lower strata we have especially those of higher atomic weight, all together forming a so-called "reversing layer" by which chiefly the Fraunhofer spectrum is produced.

The new hypothesis necessitates a radical change in the above views. According to it the three main statements just made require to be changed as follows :—

(1.) If the terrestrial elements exist at all in the sun's atmosphere they are in process of ultimate formation in the cooler parts of it.[2]

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[1] See *ante*, Chapter XIII.

[2] See *ante*, p. 201 at bottom.

(2.) The sun's atmosphere is not composed of strata which thin out, all substances being represented at the bottom; but of true strata, like the skins of an onion, each different in composition from the one either above or below. Thus, taking the sun in a state of quiescence and dealing only with a *section,* we shall have (as shown in fig. 101) C, say, containing neither D nor B, and B containing neither A nor C.

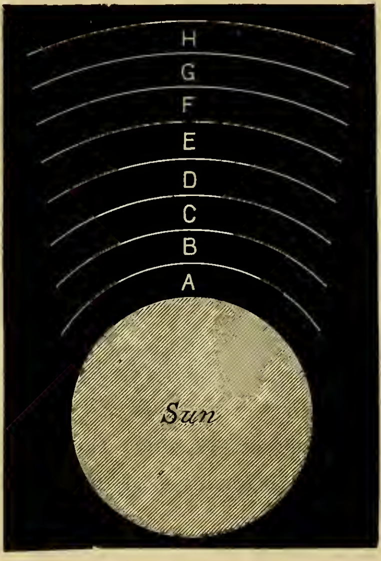

FIG. 101.—Layers in solar atmosphere.

(3.) In the lower strata we have not elementary substances of high atomic weight, *but those constituents of the elementary bodies which can resist the greater heat of these regions.*

The conditions under which we observe the phenomena of the sun's atmosphere have not, as a rule, been sufficiently borne in mind, and it is quite possible that the notion of the strata thinning out has, to a certain extent, been based more upon the actual phenomena than upon reasoning upon the phenomena.

I have referred to this point before, p. 164, but recur to it here for greater clearness.

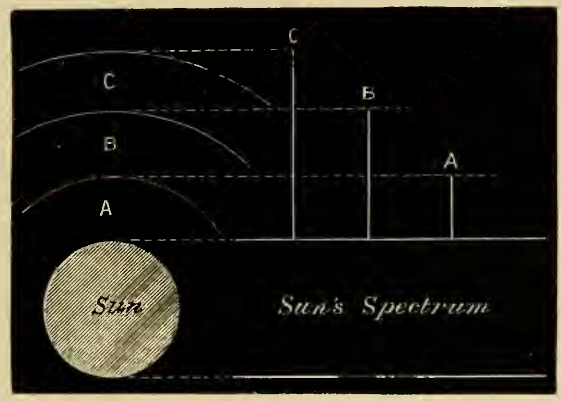

FIG. 102.—Lines produced by layers are of different lengths.

Take three concentric envelopes of the sun's atmosphere, A, B, C (fig. 102), so that C extends to the base of A, and B also to

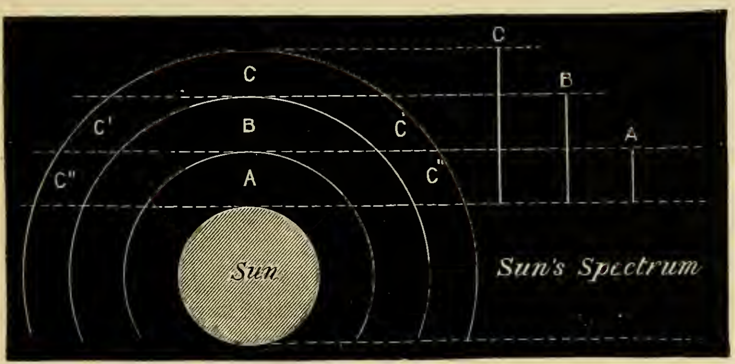

FIG. 103.—But lines of different length do not necessarily indicate that the substances producing the longer of them extend down to the photosphere.

the base of A, that is, in both cases to the photosphere. Then, whether we deal with the sphere or with a section of it, the lengths of the lines in the spectrum of the strata C, B, A, will give the heights to which the strata extend from the sun, and show where B and A respectively thin out. As the material is by hypothesis continuous down to the sun, the lines will be continuous down to the spectrum of the sun seen below as shown.

Now take three concentric envelopes, A, B, C (fig. 103), so that only A rests on the photosphere, B rests on A, and C on B. The phenomena will *in the main* be the same as in the former case, *i.e.*, the line C will still appear to rest on the spectrum of the photosphere, for it will be fed, so to speak, from C′ and C″, though absent along the line CBA at B and A. So also with B.

Thus much having been premised with regard to the observations as conditioned by the fact that we are observing a sphere, we can now proceed to note *how the two hypotheses deal with the facts.*

Old Hypothesis.	*New Hypothesis.*
1. The spectrum of each element as seen in our laboratories should be exactly represented in the solar spectrum.	The spectra should *not* resemble each other.

FACT.—There is a very wide difference between the spectra. See *ante*, pp. 174 and 230.

2. The spectrum of the base of the solar atmosphere should most resemble the ordinary Fraunhofer spectrum.	The spectrum of the base should least resemble the Fraunhofer spectrum, because at the base we only get those molecules which can resist the highest temperatures.

FACT.—When we leave out of consideration the lines of hydrogen, calcium, and magnesium, those seen at the base, as a rule, are either faint Fraunhofer lines, or are entirely absent from the ordinary spectrum of the sun. See p. 182.

3. The spectra of prominences should consist of lines familiar to us in our laboratories, and should have the same intensities, because solar and terrestrial elements are the same.	The spectra of prominences should even be in many cases unfamiliar, because prominences represent what is going on at a temperature hot enough to prevent the coming together of the atoms of which our chemical elements are composed.

FACT.—When we leave out of consideration the lines of hydrogen, calcium, magnesium, and sodium, which are seen in the hottest stars, most of the lines are either of unknown origin or are feeble lines in the spectra of known elements. See p. 185.

4. *Quâ* the same element the lines widest in spots should always be the same.	*Quâ* the same element the lines widest in spots should vary enormously, because the absorbing material is likely to originate in and to be carried to different depths.

FACT.—There is immense variation, as will be seen in the sequel.

5. The spectrum of iron in a prominence should be the same as the spectrum of iron in a sun-spot.	The spectrum of iron in a prominence should be vastly different from the spectrum of iron in a sun-spot because the spot is cooler than the prominence.

FACT.—The spectra, as will be seen in the sequel, are as dissimilar as those of any two elements.

6. The spectra of spots and prominences should not vary with the sun-spot period.	The spectra should vary, because the sun is hotter at maximum.

FACT.—They do vary, as will be shown in the sequel.

7. Motion in the iron vapour, *e.g.* in a spot or a prominence, should be indicated by the contortion of all the iron lines equally.	Motion should be unequally indicated, because the lines are due to divers constituents which exist in different strata according as they can resist the higher temperatures of the interior regions.

FACT.—The indications show both rest and motion. See *post*, chap. xxiv.

From the above sketch, hasty though it be, it is, I think, easy to gather that the new view includes the facts much better than the old one, and in truth demands phenomena, and simply and sufficiently explains them, which were stumbling-blocks and paradoxes on the old one.

But we go still more into detail.

Let us first suppose, to take the simplest case, that the sun when cold will be a solid mass of one pure element, *i.e.*, that the evolution brought about by reduction of temperature shall be along one line only. Let us take iron as the final product.

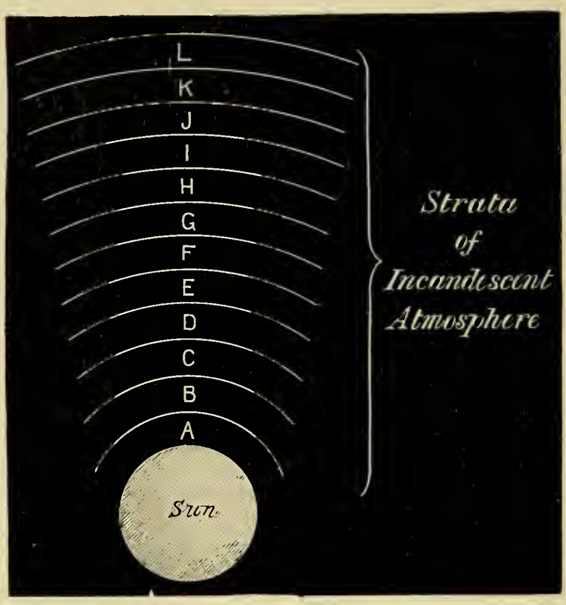

FIG. 104.—Hypothetical section of solar atmosphere.

Then the sun's atmosphere on the new theory *quâ* this one element may be represented as follows (see fig. 104):—

Assume strata A—L. Then—

(1.) The Fraunhofer spectrum will integrate for us the absorption of all strata from A to L.

(2.) The darkest lines of the Fraunhofer spectrum will be those absorbed nearest the outside of the atmosphere.

(3.) We shall rarely, if ever, see the darkest lines affected in spots and prominences.

(4.) The germs of iron are distributed among the various strata according to their heat-resisting properties, the most complex at L, the least complex at A.

(5.) Whatever process of evolution be imagined, as the temperature runs down from A to L, whether A, 2A, 4A; or A+B, 2(A+B); or A+B+C; the formed material or final product is the work of the successive associations rendered possible by the gradually lowering temperature of the successive strata, and can therefore only exist at L.

CHAPTER XXIII.

MORE TESTS. THE SPECTRA OF SUN-SPOTS.

In the last chapter when endeavouring to show how the new hypothesis fits the facts better than the old one there were some points on which I promised to give further information depending upon later work. This I now proceed to do with regard to those which refer to spot-spectra, or to the comparison of such spectra with those of the prominences.

The first attempt I made[1] to get light out of this inquiry was one which dealt with a long catalogue of lines observed by Professor Young in his memorable expedition to Mount Sherman, where, at the height of between 8,000 and 9,000 feet, with perfect weather and admirable instrumental appliances, about a month was employed in getting such a catalogue of lines as had never been got before. But it was found that, although the result of this inquiry was to strengthen the evidence in favour of the inversions referred to at p. 175, still, after all, one wanted more facts. In this changeable climate I soon found that it would not do to proceed as I began—to attempt to observe all the lines acted upon in a solar spot. The excessive complication, and the great variation of a spot-spectrum from the ordinary solar spectrum, cannot be better shown than by the accompanying copy of a spectrum of one of the sun-spots observed at Greenwich.

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[1] Proc. R. S. No. 197, 1879, page 251 and *seq.*

The figure (Fig. 105) shows a limited part of the solar spectrum, and the lines thickened in the spot-spectrum. It will be seen therefore that to tabulate the existence, thickness and intensity of these lines over the whole of the solar spectrum would be a work which it would be difficult to accomplish in a single day even if the day were absolutely fine. So that was given up in favour of a limited inquiry over a small part of the solar spectrum; limited further by this, that we only note the twelve lines most affected in each spot on each day. In this way we insure a considerable number of absolutely comparable observations and we can more easily compare the spot results with those which have been obtained in the observation of the brightest lines in prominences; because when one begins to observe lines in the solar prominences one naturally observes the brightest lines first. So that by observing the darkest lines first in the case of spots one has a fairer comparison.

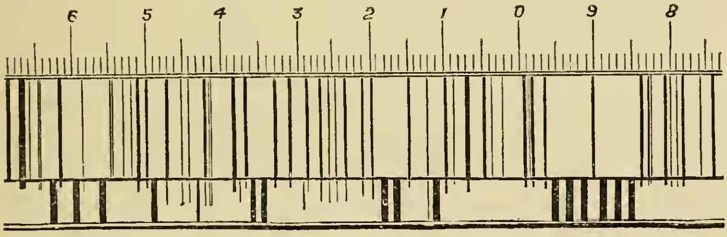

Fig. 105.—Part of the spectrum of a sun-spot observed at Greenwich.

The work in question has now been going on uninterruptedly since 1879. From the first I felt it was crucial, and that I was in duty bound to proceed with it to see if it would supply us with any additional tests.

The work enables us to discuss the lines of several chemical elements most widened in 700 spots observed at Kensington. The period of observation commenced November, 1879, and extended to August, 1885. It includes, therefore, the sun-spot curve from a minimum to a maximum and some distance beyond.

I may begin by stating the way in which the observations have been made. The work, which has been in large part done by my assistants and chiefly by Messrs. Lawrance and Greening, simply consists of a survey of the two regions F—*b* and *b*—D.

The most widened line in each region—not the widest line, but the *most widened*, is first noted; its wave-length being given in the observation books from Ångström's map. Next the lines which most nearly approach the first one in widening are recorded, and so on till the positions of six lines have been noted, the wave-lengths being given for each region, from Ångström's map.

It is to be observed that these observations are made without any reference whatever to the origin of the lines; that is to say, it is no part of the observer's work to see whether there are metallic coincidences or not; this point has only been inquired into in the reductions. In this way perfect absence of all bias is secured. It may further be remarked that the number of lines widened at any time during a sun-spot period is about the same, so that the conditions of observation vary very little from month to month, or from year to year.

The absolute uniformity of the results obtained in the case of the chemical elements investigated indicates, I think, that the observations have been thoroughly well made; and, as a matter of fact, they are not difficult.

It was not only important to record these observations for their own sake, but also for the comparison they allowed us to make with other observations of different phenomena, especially of those presented by the prominences. Side by side with the sun-spot work, therefore, went on an elaborate mapping of every prominence line observed by Tacchini since 1870. The reason of this is obvious, but will bear repetition here. The spots, as everybody agrees, are caused by down currents when a disturbance in the solar atmosphere brings vapour down from the cooler exterior regions. The prominences, on the other hand, are either ejections from the most highly heated part of the sun below the photosphere, or are caused by the dissociation of the descending material when it has reached the point of highest temperature.

This being premised we pass on to the results obtained by the spot observations, including with them the results of the comparison with the prominence spectra. I will begin with the results obtained by the complete discussion of the first hundred observations taken between November, 1879, and September, 1880. Each line observed in each spot was carefully mapped on a large scale on sheets on the top of which were shown the Fraunhofer lines with their true intensities in the region explored.

General Statements regarding Spots.

1. The spot-spectra are very unlike the ordinary spectrum of the sun, some Fraunhofer lines are omitted, new lines appear and the intensities of the old lines are changed.

2. The next point was that in the case of each chemical element, even those with many lines among the Fraunhofer lines, only a very few lines, comparatively speaking, were seen to be most widened. It was as if on a piano only a few notes were played over and over again, always producing a different tune.

3. An immense variation, from spot to spot, was observed between the most widened lines seen in the first hundred observations. Change of quality or density will not account for this variation. To investigate this point I had the individual observations of lines seen in the spectrum of iron plotted out on strips of paper, and I then tried to arrange them in order, but I could not succeed, for even when the observations were divided into six groups about half of them were left outstanding.

4. If we consider the lines of any one substance, there is as much inversion between these lines as between the lines of any two metals. By the term inversion I mean of any three lines,

A, B, C, that we may get A and B without C, A and C without B, B and C without A, and so on.

One was struck with the marvellous individuality, so to speak, of each of the lines. They did not go in battalions, or companies, or corporal's guards, but in single unities. Each in turn is seen without the other. We get as much inversion of lines in the case of one element as we could do between the lines of different elements; by which I mean that the lines of nickel, say, are just as much varied in different spots as the lines of iron, nickel or calcium would be in spots in which the proportions of these substances very greatly varied.

5. Very few lines, indeed, are strongly affected at the same time in the same spot. A great many lines of the same substance may be affected, of course, besides the twelve recorded as most widened on each day; but a small number relatively altogether are affected in this manner.

6. Many of the lines seen in the spots are lines seen at low temperatures (some of them in the oxyhydrogen flame), and none of them are those brightened or intensified when we pass from the temperature of the electric arc to that of the electric spark.

7. Certain lines of a substance have indicated rest, while other adjacent lines seen in the spectrum of the same substance in the same field of view have shown change of wave-length.

8. A large number of the lines seen in spots are common to two or more substances with the dispersion employed.

9. The lines of iron, cobalt, chromium, manganese, titanium, calcium, and nickel seen in the spectra of spots are usually coincident with lines in the spectra of other metals, with the dispersion employed; whilst the lines of tungsten, copper, and zinc seen in spots are not coincident with lines in other spectra.

10. The lines of iron, manganese, zinc, and titanium most frequently seen in spots are different from those most frequently seen in flames, whilst in cobalt, chromium, and calcium the lines seen in spots are the same as those seen in flames.

11. Towards the end of the first series a few lines appeared among the most widened ones which are not represented, so far as is known, among the lines seen in the spectra of terrestrial elements. This change took place when there was a marked increase in the solar activity.

A second series was begun on 29th September, 1880, and came down to October, 1881. The second series, like the first one, consisted of 100 observations, these observations being either of different spots or of the same spots seen on different days, and therefore in different stages of development.

The following results were obtained:—

12. The number of new lines seen amongst the most widened lines steadily increased. Many of these lines are very faint in the solar spectrum, and are unrecorded by Ångström, while they are wide and dark in the spot-spectrum.

13. In the months of May and June (1881), there was a great change in the spectra of the spots, the old lines dying out and new lines appearing.

14. When series of observations, consisting of ten consecutive observations of the spectra of spots, taken from the commencement of the first series in November, 1879, and from the end of it on 27th September, 1880, were compared with those made towards the end of the second series on 18th July, 1881, it was found that the lines widened in each set were markedly distinct from those in the other sets.

To illustrate this, I give the diagram on page 316. At the top are some of the principal Fraunhoferic lines in the region F to D, the lengths representing the intensities. The lower part of the diagram is divided into three sections by strong lines; the first of these (1—10) contains the observations made between November 12, 1879, and January 20, 1880, the second (11—20) the observations made between September 27 and October 1, 1880, and the third those made between July 18 and July 29, 1881.

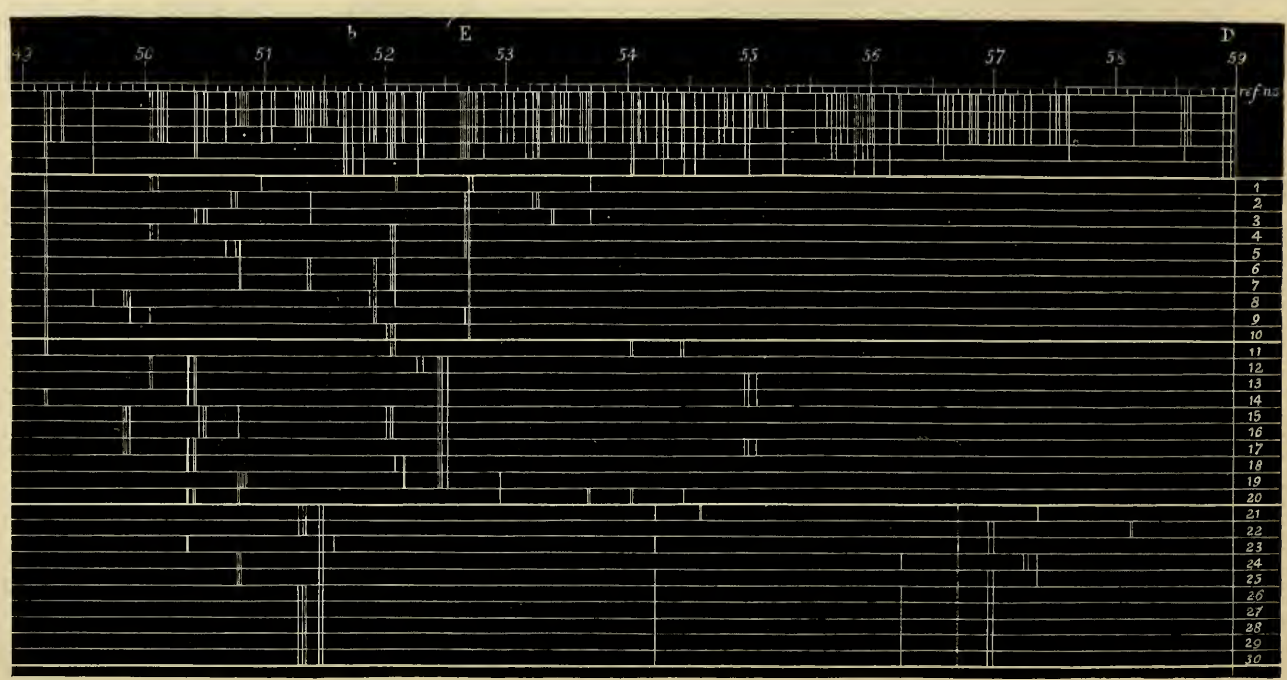

FIG. 106.—Map showing the most widened lines observed in sun-spots in ten observations taken from three different periods—about January 1880, September 1880, and July 1881. [It will be observed that there is almost a complete change from the first period to the third.]

15. At the commencement of October, 1881, there was a change in the spectra of the spots similar to that which took place in May and June, but much more abrupt, for only one of the old lines remained. This is exactly analogous to variations observed by Tacchini in the spectra of the prominences in the region F to *b*, in December, 1872.

16. In the first hundred observations the total number of most widened lines in the region F to *b* was fifty-seven, forty of which were due to iron, whilst in the second hundred the total number of lines seen was 104, and the iron lines faded away gradually, the last disappearing on 26th July, 1881. By November of that year more than 75 per cent. of the most widened lines are not represented in the spectra of terrestrial elements.

The result of this inquiry with regard to chemical substances which has been most carefully worked out, is indicated in the accompanying table, giving the result of the work for two years from 200 spots.

STATISTICS OF THE MOST WIDENED LINES SEEN IN 200 SUN-SPOTS.

	Total number of lines in part of spectrum discussed.	Total number of lines widened.
Iron	172	72
Titanium	120	38
Nickel	24	9
Zinc	19	5
Cobalt	17	3
Calcium	17	7
Chromium	15	9
Molybdenum	14	1
Tungsten	14	2
Manganese	13	4
Platinum	12	1
Barium	10	1
Copper	10	1
Sodium	7	2

In these 200 spots, out of 172 lines of iron which we might have seen only 72 were observed altogether; out of 120 lines of titanium which we might have seen only 38 were seen; and then the number goes on decreasing: 24 in the case of nickel, of which 9 were seen; 19 in the case of zinc, of which 5 were seen; 13 of magnesium, of which 4 were seen; 12 of platinum, of which 1 was seen, and so on.

The final upshot is, therefore, that at the spot-level we do not see the Fraunhofer spectrum, as we ought to do on the old theory. What we do see is a small percentage of the lines, and we see them under conditions which are entirely unexpected.

These, though the earlier results, are not the only ones which we may hope to get by going on with the work. At present the dates of the spots have alone been recorded. But this is not enough; we must know the actual positions of the spots on the sun. We must note whether each particular spot is in the northern hemisphere or in the southern hemisphere, with the view of determining whether there is any chemical difference between the north part of the sun and the south part; and then again we must compare the latitudes of spots, with the view of determining whether there is any difference in the chemistry of the spots according to the latitude. This particular point is just now being worked up, and it really does look as if the sudden changes in the spectra noted from time to time may have been due to the fact that the spots compared were spots varying very considerably in latitude, and it would not surprise me to find that spots which are very like each other in their spectra will be found to be situated more or less in the same degree of latitude,—whether the same degree of latitude north or south we do not know. And there is another question, too. I pointed out that there is a considerable number of lines seen in the spectrum of the arc which are left out of the spectrum of the spark. Now, will that help us at all in our inquiries? I think perhaps it may in time, and that we shall ultimately be able, in the way indicated, to classify spots according to their temperature.

Although the reduction of the whole 700 observations is not yet complete, some of the general results obtained from the whole of the observations up to 1885 may be briefly referred to in this place.

I first give tables (A, B, C) showing that for each of the chemical elements so far considered—iron, nickel, and titanium—the number of lines seen in the aggregate in each hundred observations is reduced from the sun-spot minimum to the maximum, and this result holds good for both regions of the spectrum.

I give another table (D) showing that during the observations the lines recorded as most widened near the maximum period of sun-spots have not been tabulated amongst metallic lines by either Ångström or Thalèn, and that many of them are not among the mapped Fraunhofer lines, though some of them may exist as faint lines in the solar spectrum when the observing conditions are best.

The result of the observations of 700 spots—observations extending over six years—along this line, may be thus briefly stated. As we pass from minimum to maximum, the lines of the chemical elements gradually disappear from among those most widened, their places being taken by lines of which at present we have no terrestrial representatives. Or, to put the result another way—at the minimum period of sun-spots when we know the solar atmosphere is quietest and coolest, vapours containing the lines of some of our terrestrial elements are present in sun-spots. The vapours, however, which produce the phenomena of sun-spots at the sun-spot maximum are entirely unfamiliar to us.

TABLE A.—IRON.

Iron Lines observed in Sun-spot Spectra among the most Widened Lines.

	4863·2	4870·2	4871·3	4875·5	4877·4	4884·2	4886·5	4888·0	4890·0	4890·6	4907·0	4909·5	4918·0	4919·8	4956·7	4981·8	4982·5	4983·5	4984·8	5004·9
1st HUNDRED. 12th Nov., 1879, to 29th Sept., 1880.		│	│		│			│	│	│	│		│	│	│	│	│	│	│	│
2nd HUNDRED. 29th Sept., 1880, to 15th Oct., 1881.	│	│	│	│		│	│	│	│	│			│	│	│	│	│	│	│	
3rd HUNDRED. 18th Oct., 1881, to 27th June, 1882.																				
4th HUNDRED. 1st July, 1882, to 28th August, 1883.		│	│						│	│		│								
5th HUNDRED. 30th August, 1883, to 23rd June, 1884.		│	│						│				│	│						
6th HUNDRED. 24th June, 1884, to 12th Feb., 1885.																				
7th HUNDRED. 19th Feb., 1885, to 24th August, 1885.											│	│								

	5005·2	5006·5	5011·5	5014·2	5019·2	5026·2	5027·2	5038·2	5040·1	5041·2	5047·8	5049·4	5051·0	5064·4	5068·1	5071·9	5074·0	5075·8	5077·9	5078·8
1st HUNDRED. 12th Nov., 1879, to 29th Sept., 1880.	│	│	│						│	│	│	│	│	│	│	│	│	│	│	│
2nd HUNDRED. 29th Sept., 1880, to 15th Oct., 1881.			│	│	│	│	│	│	│	│	│	│	│		│	│	│	│	│	│
3rd HUNDRED. 18th Oct., 1881, to 27th June, 1882.								│											│	│
4th HUNDRED. 1st July, 1882, to 28th August, 1883.									│	│									│	│
5th HUNDRED. 30th August, 1883, to 23rd June, 1884.								│												
6th HUNDRED. 24th June, 1884, to 12th Feb., 1885.																			│	│
7th HUNDRED. 19th Feb., 1885, to 24th August, 1885.																				

	5082·2	5090·2	5096·3	5098·2	5107·0	5109·8	5121·0	5123·2	5126·5	5133·0	5136·8	5138·6	5141·8	5145·7	5150·0	5151·2	5158·5	5161·5	5165·8
1st HUNDRED. 12th Nov., 1879, to 29th Sept., 1880.			│	│	│		│				│	│	│	│					│
2nd HUNDRED. 29th Sept., 1880, to 15th Oct., 1881.	│	│		│	│	│		│			│	│	│	│	│	│	│	│	
3rd HUNDRED. 18th Oct., 1881, to 27th June, 1882.	│						│	│	│	│	│			│	│	│			│
4th HUNDRED. 1st July, 1882, to 28th August, 1883.	│				│									│	│		│	│	
5th HUNDRED. 30th August, 1883, to 23rd June, 1884.										│									
6th HUNDRED. 24th June, 1884, to 12th Feb., 1885.										│									
7th HUNDRED. 19th Feb., 1885, to 24th August, 1885.										│									

Table B.—Nickel. List of most Widened Lines observed.

	4865·3	4872·5	4903·9	4917·6	4935·1	4979·6	4983·5	5016·8	5034·6	5079·8	5080·6	5098·5	5099·2	5114·9	5136·8	5141·8	5145·7	5155·1	5168·3	5175·6
1st hundred spots							│		│						│	│	│			
2nd hundred spots							│	│	│	│	│	│			│	│	│			
3rd hundred spots										│	│				│		│	│		
4th hundred spots										│	│						│	│		
5th hundred spots																		│		
6th hundred spots											│									
7th hundred spots																		│		

Table C.—Titanium. List of most Widened Lines observed.

	4869·5	4884·2	4913·2	4964·5	4981·0	5006·6	5013·3	5035·2	5035·8	5037·8	5038·0	5038·7	5052·3	5061·3	5064·4	5071·8	5086·5	5119·9	5126·6	5144·5	5147·0	5151·2
1st hundred spots					│	│		│	│						│	│						
2nd hundred spots	│	│	│	│	│		│	│	│	│		│	│	│		│	│					│
3rd hundred spots								│	│	│		│		│	│			│	│		│	│
4th hundred spots									│		│			│						│	│	
5th hundred spots										│	│									│	│	
6th hundred spots																				│		
7th hundred spots	No lines.																					

TABLE D.—Unknown Widened Lines observed.

	1st Hundred.	2nd Hundred.	3rd Hundred.	4th Hundred.	5th Hundred.	6th Hundred.	7th Hundred.
4865	...	...	...	...	...	1	...
4885	...	...	...	...	...	...	1
4888·3	1	...	...	...	...	...	...
4891·8	...	...	...	...	1	...	...
4910	...	...	...	2	...	...	...
4944	...	...	1	...	...	...	...
5017·2	...	1	...	...	...	...	...
5028·9	...	1	...	...	...	...	...
5030	...	1	...	...	...	...	...
5034·8	11	...	3	...	...	...	...
5037	...	...	...	...	...	1	...
5038·9	...	...	1	1	...	...	...
5042	...	...	3	...	...	...	..
5042·3	...	...	4	...	...	...	...
5043	...	1	...	...	...	...	...
5044·6	...	3	...	...	...	...	...
5061	...	...	2	...	...	...	3
5061·5	...	...	...	...	...	...	2
5062	...	...	...	...	...	...	5
5062·4	...	...	...	...	...	2	...
5062·8	...	...	...	3	...	2	...
5065	...	...	8	...	...	...	...
5067	...	1	...	...	...	...	...
5069·5	...	1	...	...	...	...	...
5070·8	...	1	...	...	...	...	...
5077	...	...	...	1	...	...	...
5079·5	...	...	...	...	...	2	...
5080	...	...	...	I	...	...	...
5081·5	...	...	...	...	...	...	3
5082	...	...	...	...	...	2	...
5083	...	...	...	...	...	2	...
5083·3	...	1	2	3	...	...	...
5084	...	1	...	...	...	...	3
5084·5	...	...	...	...	...	...	2
5086	...	...	17	...	1	...	...
5086·8	...	1	...	...	...	...	...
5087·7	...	1	...	...	...	...	...
5088·1	...	1	...	...	...	...	...
5088·6	...	1	...	...	...	...	...
5089·0	...	...	...	1	...	...	...
5101	..	...	...	...	...	...	1
5103·5	...	...	...	...	1	...	...
5112·1	...	6	22	4	2	1	...
5115·5	...	...	...	...	...	...	9
5116	3	6	24	3	...	...	...
5116·2	...	...	7	...	...	...	. .
5118	4	...	14	...	...	...	...
5127	...	...	...	1	...	...	...
5127·5	...	...	1	...	..	...	...

	1st Hundred.	2nd Hundred.	3rd Hundred.	4th Hundred.	5th Hundred.	6th Hundred.	7th Hundred.
5128·8	...	...	...	...	...	...	1
5129·6	...	17	19	4	...	...	...
5130	...	...	1	...	...	...	...
5132	...	14	21	6	...	...	...
5132·5	...	...	1	...	...	...	...
5132·8	...	...	...	3	...	...	...
5133·5	...	...	1	...	1	3	17
5133·8	...	30	47	43	62	3	27
5134	...	...	...	...	12	41	10
5134·4	...	...	...	...	...	19	...
5135	...	...	...	...	16	36	11
5135·5	...	33	15	...	53	36	20
5135·8	...	...	37	52	13	2	...
5136	...	...	4	...	9	22	27
5136·5	...	...	3	1	...	...	...
5137	...	...	2	2	1	...	...
5137·5	...	...	4	...	72	79	22
5137·8	...	12	35	64	13	10	3
5138	...	...	...	...	1	...	3
5139	...	...	...	...	1	...	1
5139·4	...	1	2	3	...	...	...
5140·4	...	2	...	...	...	...	...
5142·2	13	4	...	1	...	...	...
5142·8	...	21	7	19	2	...	...
5143	...	...	...	...	...	...	20
5143·2	...	...	2	...	...	...	...
5144·2	1	3	...	2	...	...	...
5144·5	...	...	...	...	1	...	...
5145·5	...	...	...	...	...	1	...
5146	...	...	36	12	...	...	...
5146·5	...	...	...	2	...	...	...
5148	...	...	...	...	...	...	1
5148·8	...	...	1	2	...	...	...
5149	2	32	31	36	4	...	35
5149·2	...	...	...	1	...	...	...
5149·5	...	...	...	4	...	...	29
5149·8	...	8	2	8	...	8	...
5150	...	...	...	1	...	...	...
5151·8	...	...	...	...	1	...	...
5153·8	...	...	...	...	1	...	...
5154	...	...	...	...	...	...	1
5155·4	...	...	...	...	1	...	...
5156	1	12	37	74	32	91	95
5156·5	...	...	...	...	8	...	...
5157·2	...	...	...	4	...	...	...
5159	...	...	1	8	13	11	41
5159·5	1	...	31	59	80	86	57
5160	...	...	1	4	...	9	...
5160·4	...	1	...	5	...	...	4
5162	...	...	9	7	61	67	62
5162·2	1	...	23	49	21	30	...
5175	...	...	...	...	...	...	3

The disappearance of the lines of iron, nickel, and titanium, and the appearance of unknown lines as the maximum is reached is shown by curves in Fig. 107 given on the next page.

The important results thus obtained from the preliminary discussion of the whole of the 700 spots enable us to see that the strange changes noted in the second series (see page 315) were but the indications of the effects of a general law which connects the lines affected in spots with the sun-spot period, and obviously with the varying temperatures proper to each phase of the period.

Hence we may now generalise the last six statements given on page 316 as follows:—

12. The most widened lines in sun-spots change with the sun-spot period.

13. At and slightly after the minimum, the lines are chiefly known lines of the various metals.

14. At and slightly after the maximum, the lines are chiefly of unknown origin.

15. On the hypothesis under discussion the change indicates an increased temperature in the spots at the sun-spot maximum.

The results, in my opinion, amply justify the working hypothesis as to the construction of the solar atmosphere given in chapter xxii.

I am quite content, therefore, to believe that iron, titanium, nickel, and the other substances very nearly as complex as we know them here, descend to the surface of the photosphere, in the downrush that forms a spot at the period of minimum; but that at the maximum, on the contrary, only their finest constituent atoms can reach it. It may also be remarked that these particles which survive the dissociating energies of the lower strata are not the same particles among the constituents of the chemical elements named which give the chromospheric lines recorded by Tacchini, Ricco, and myself.

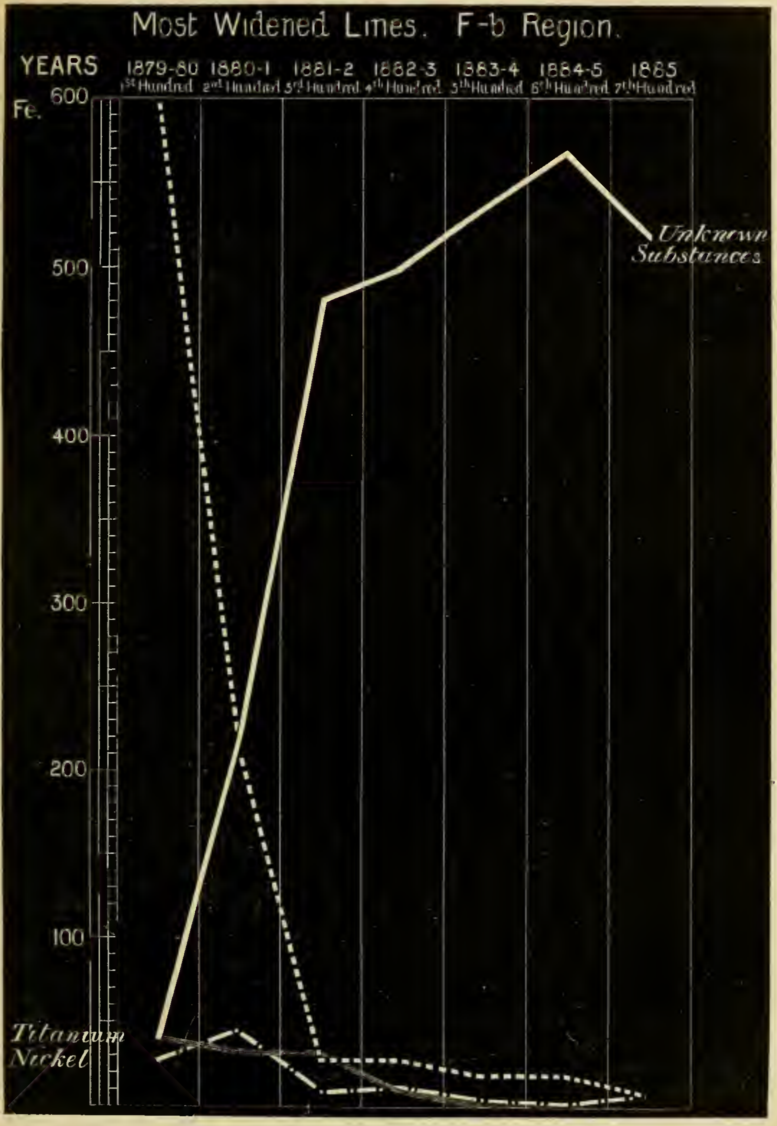

FIG. 107.—Number of appearances of known and unknown lines.

CHAPTER XXIV.

SPECIAL TESTS WITH REGARD TO IRON.

1. *Young's Work.*

In the last chapter I gave the sun-spot observations in their most general form, represented by Fig. 106, in which we had the Fraunhofer lines compared with the lines widened in spots.

Now if I had contented myself with such maps as that a portion of which was selected to engrave the figure in question, it might, and it certainly would have been said that nothing was more natural, that in fact the only result which had been got out, was that there was more of one chemical element in one spot and more of another in another; and that the *inversions* were simply due to this cause. I have already shown, however, that we have got far beyond that, and that the inversions are truly inversions of lines of the same metal. This is one of the points to be strengthened in the present chapter.

These results have been mapped in all their details in another long series of maps of a different kind, the difference being that instead of considering all the Fraunhofer lines and all the lines recorded in spots and prominences, we have a map for each hundred observations, strictly limited, both as regards sun-spot and prominences, to one metal.

I propose in this chapter to refer specially to this latter series of maps.

Part of the work which has been undertaken in connection with this special branch of the investigation in order to enable us to add laboratory results to the solar ones, has been a careful inquiry into the changes brought about in the spectrum of substances by exposing them to widely different temperatures. In the case of iron our last work has been to note the lines visible when fine iron gauze is burned in the flame produced by a mixture of oxygen and coal gas. The research is a very laborious one, and it may be that some day we shall get a very much better record than that which my assistants and myself have produced; what we have been able to do we have done over the region of the spectrum already worked over in the spots and flames.

In any case it is clear that when such facts as these have been studied in the laboratory, we can then compare both solar and laboratory evidence. When these observations are taken into account, we can not only deal with the sun-spot observations comparing them with the spectra of prominences, but with the changes of spectra observed in our laboratories when various temperatures are employed.

And further, we here limit ourselves to one spectrum, that of iron, for the reason that it has been studied with the greatest care by several eminent observers. In this way we shall be best brought face to face with the actual phenomena, and with very definite statements of facts over a small area. *Dolus latet in generalibus.*

Many of the points brought out strongly by the recent spot observations may be gathered, though not certainly in all their fulness, from a comparison of the spot and prominence observations made by Young at Sherman so far back as 1872. I will therefore give them here. I have brought both sets of observations together in the following table, and it will be seen what small relation there is between the intensities of the lines seen in our laboratories and the number of times they are seen, either in spots or storms; while the fact that many lines affected in spots are not seen in prominences, and *vice versâ*, comes out very clearly.

THALÉN.			YOUNG.				
			Spots.		Prominences.		
Wave-lengths of lines.	Intensitie	Coincident with.	If visible.	Widening.	If visible.	How many times.	Brightness.
6489·8	3						
6407·0	Map	...	...	...	...	...	...
6399·0	1	...	...	...	Yes	5	2
6392·6	Map	...	...	...	Yes	5	1
6357·7	Map	...	Yes	4			
6300·3	3						
6245·4	2	...	...	...	Yes	8	5
6231·5	Map	...	...	...	Yes	5	1
6229·7	2						
6199·6	Map	...	...	...	Yes	2	2
6190·5	2	...	Yes	2	Yes	10	2
6148·1	Map	...	...	...	Yes	3	2
6135·6	2	...	Yes	3	Yes	2	1
6064·5	2	Ti	Yes	3	Yes	5	2
6023·0	3						
6019·1	4						
6007·5	4	...	Yes	...*	..	...	...
6002·1	4						
5986·2	4						
5984·2	4						
5982·8	4						
5976·1	4						
5974·6	4						
5913·2	Map	...	...	...	Yes	2	1
5883·0	Map	...	...	...	Yes	2	1
5761·9	3						
5708 3	3	...	...	...	Yes	1	1
5705·1	Map	...	Yes	4	...	...	...
{ 5681·5	Map	...	...	...	Yes	2	1 }
5681·4	3	Na	...	...	...	...	...
5661·5	3	Ti	Yes	4	Yes	15	2
5657·6	1	...	...	...	...	...	...
5654·4	3	...	...	...	Yes	2	1
5623·2	3	...	Yes	2	Yes	2	1
5614·5	1	...	Yes	3	Yes	2	1
5601·7	1	Ca	Yes	2			
5597·2	1	Ca	Yes	2			
5591·2	2	...	Yes	2			
{ 5585·6	1						
5585·5	Map	...	Yes	3	Yes	2	1
5583·7	Map	...	Yes	4			
5574·9	2						
5571·7	1	...	Yes	2			
5568·5	2						

* Double nearly invisible in spot spectrum.

Thalén.			Young.				
			Spots.		Prominences.		
Wave-lengths of lines.	Intensities.	Coincident with.	If visible.	Widening.	If visible.	How many times.	Brightness.
5531·6	Map	...	Yes	2			
5525·9	Map	...	...	...	Yes	40	5
5505·9	3						
5500·5	3	...	Yes	2	Yes	2	1
5496·6	3	...	Yes	2	Yes	2	1
5486·8	4	...	Yes	3			
5462·3	Map	...	...	...	Yes	1	1
5454·7	1	...	Yes	3	Yes	10	4
5445·9	1	...	Yes	4	Yes	10	4
5433·0	Map	...	Yes	4	Yes	2	2
5428·8	1	...	Yes	3	Yes	8	3
5423·6	Map	...	Yes	3			
5414·5	Map	...	Yes	3	Yes	2	2
5410·0	Map	...	...	...	Yes	2	1
5404·8	2	...	Yes	4	Yes	2	1
5403·1	2	Ti	Yes	4	Yes	5	3
5396·1	2	Ti	Yes	7	Yes	4	2
{ 5392·3	3						
{ 5392·2	Map	...	...	...	Yes	2	1
5382·3	3						
5370·5	1	...	Yes	4	Yes	10	3
5369·0	3	...	Yes	4	Yes	1	1
5366·5	3	...	...	...	Yes	1	1
5364·0	3	...	...	...	Yes	1	1
5361·9	4	...	...	...	Yes	20	10
5352·4	4	Co	Yes	2	Yes	4	2
5348·6	4						
5340·2	2	Mn	Yes	2	Yes	1	2
5339·2	2	...	Yes	2			
5327·6	Map	...	...	...	Yes	5	2
{ 5327·3	1	...	Yes	2			
{ 5327·1	Map	...	...	...	Yes	5	2
5323·4	2						
5315·9	2	...	Yes	...*	Yes	90	50
5306·5	3						
5301·5	3						
5282·6	2						
5280·9	3						
5269·5	1	...	Yes	3	Yes	15	4
5268·5	1	...	Yes	3	Yes	12	3
5265·8	2	Co	Yes	2	Yes	10	4
5262·4	4						
5254·1	Map	...	...	...	Yes	1	2
5249·7	Map	...	...	...	Yes	3	1

---

* Distinctly weakened and sometimes reversed.

THALÉN.			YOUNG.				
			Spots.		Prominences.		
Wave-lengths of lines.	Intensities.	Coincident with.	If visible.	Widening.	If visible.	How many times.	Brightness.
5246·3	Map	...	...	...	Yes	3	1
5239·0	Map	...	...	...	Yes	4	2
5232·1	1	...	Yes	2	Yes	1	3
5229·0	Map	...	Yes	2			
5226·2	1	...	Yes	2	Yes	10	3
5216·5	Map	...	...	...	Yes	2	1
5215·5	Map	...	...	...	Yes	3	2
5214·4	Map	...	...	...	Yes	2	1
5207·6	3	Cr	Yes	4	Yes	10	6
5203·7	3	Cr	Yes	4	Yes	10	6
5201·5	4	...	Yes	2	Yes	5	3
5197·9	Map	...	Yes	3	Yes	1	1
5194·1	3	...	...	...	Yes	2	2
5191·7	2	...	Yes	2			
5190·5	4	...	Yes	2			
5185·1	Map	...	...	...	Yes	5	2
5171·1	4						
5168·3	3	Ni	Yes	4*	Yes	40	30
5166·7	2	Mg	Yes	2*	Yes	30	20
5161·6	4						
5151·3	Map	...	Yes	2			

* Sometimes reversed.

The above tables then clearly indicate the considerable differences which have been noted by former observers touching the various appearances and intensities of the lines of iron as seen in sun-spots and prominences respectively; but it might be imagined that when we use the highest temperature available here and contrast the spectrum thus obtained with that given us by the prominences, that here at all events we should get a closer agreement. The map given on the next page will show that this is not so, and that there are the greatest divergences of intensity between lines seen under these two conditions; that many of the lines seen in our laboratories when a chemical element is studied and therefore

Iron in the Spark and in Prominences.

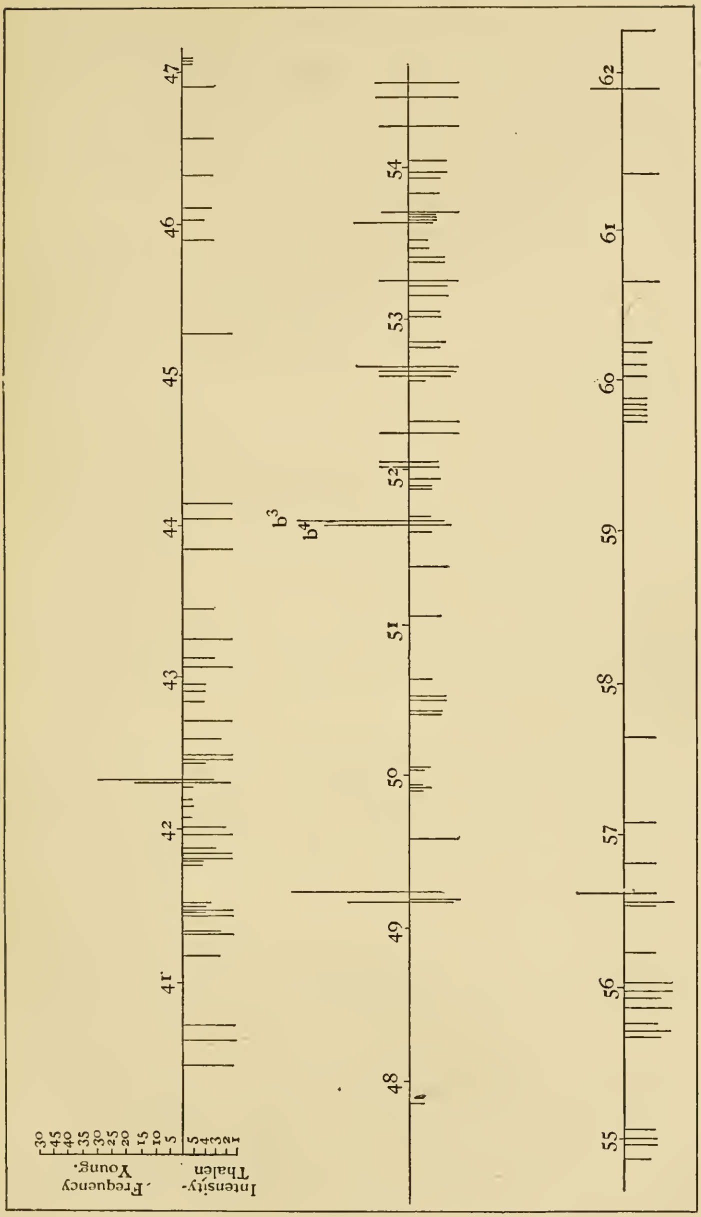

FIG. 108.—Intensities of iron lines [Thalén] compared with the iron lines seen in prominences [Young].

called by us lines of that chemical element, are absent from this part of the solar record. I show elsewhere that many of the bright lines seen in prominences are very probably lines due to descending cooler vapours, but even if we make allowance for this the divergences observed by Professor Young must still be regarded as extraordinary.

To show this in connection with Professor Young's Sherman observations of prominences; in 1879 I prepared maps of the spectra of calcium, barium, iron, and manganese. In these the lengths of the lines in the spectra of the metallic elements represent the intensities given by Thalén, whose lines and wave-lengths I have followed in all cases, while those of the lines visible in prominences, represent the number of times each line has been seen in the spectrum of the chromosphere by Professor Young. An inspection of these maps is sufficient to show that there is no connection whatever beyond that of wave-length between the spectra; it will be gathered from them how the long lines seen in our laboratories are suppressed and the feeble lines exalted in the spectrum of the chromosphere.

Of these spectrum maps I give only that of iron, although if space had permitted I should have been glad to add those of calcium and barium, because in some cases the changes of intensity in those spectra are even more striking than in iron.

2. *Some Details of the Changes of Intensity in the Iron Spectrum.*

In studying the iron spectrum we may not only consider the visible portion, but we can also include in our inquiry that recorded on my photographic plates, between H and G.

It may be described as a very complicated spectrum, so far as the number of lines is concerned, in comparison with such bodies as sodium and potassium, lead, thallium, and the like; unlike them, again, it contains no one line which is clearly and unmistakably reversed on all occasions. Compared, however, with the spectrum of such bodies as cerium and uranium, the spectrum is simplicity itself.

My attention was first directed to the variation in the relative intensity of the iron lines under different conditions by the differences easily recognised in the case of some of the triplets with which the spectrum is crowded.

These give us beautiful examples of those repetitions of structure which we meet with in the spectra of almost all bodies, some of which have already been pointed out by Mascart, Cornu and myself. In many photographs in which iron has been compared with other substances, and in others again in which iron has been photographed as existing in different degrees of impurity in other substances, these triplets have been seen almost alone, and the relative intensity of them, as compared with the few remaining lines, is greatly changed. In this these photographs resemble one I took ten years ago, in which a large coil and jar were employed instead of the arc; they necessitated an exposure of an hour instead of two minutes. In this the triplet near G is very marked; the two adjacent more refrangible lines near it, which are seen nearly as strong as the triplet itself in some of the arc photographs I possess, are only very faintly visible, while dimmer still are seen the lines of the triplet between H and *h*. In the spark photograph, then, the more refrangible triplet is barely visible, while the one near G is very strong.

Fig. 109, which is copied from a photograph, will show another difference. The lines at wave-lengths 4325·0, 4300·7, 4271·0, composing the less refrangible triplet, are three of the strongest iron lines in the arc spectrum, and those at 4071·0, 4063·0, 4045·0, composing the more refrangible ones, are less strong than the others. It will be seen that in the solar spectrum the last triplet is much more important, much thicker, and much darker than the first, so that here is an absolute inversion in the intensity of the lines. I appeal to the photograph because it is perfectly impartial; it has no view, no anxiety therefore to intensify one particular part of it at the expense of the other, and it is referred to as the exemplar of many similar reversals which we see whenever such observations are made.

One word here on the probable origin of these triplets. If one molecular grouping alone were in question, their relative intensity should always be preserved, however much the absolute intensity of the compound system might vary; but if it is a question of two molecules, we might expect that, in some of the regions open to our observation, we should get evidence of cases in which the relative intensity is reversed or the two intensities are assimilated. What might happen does happen; the intensity of the two triplets in the spark photograph is as we have seen reversed in the spectrum of the sun. The lines barely visible in the spark photograph are among the most prominent in the solar spectrum, and the triplet which is strong in that photograph is represented by Fraunhofer lines not half so thick. While the hypothesis that the iron lines in the region I have indicated are produced by the vibration of one molecule does not include all the facts, the hypothesis that the vibrations are produced by at least three distinct molecules I think does include them. There are other facts in another line of work. In

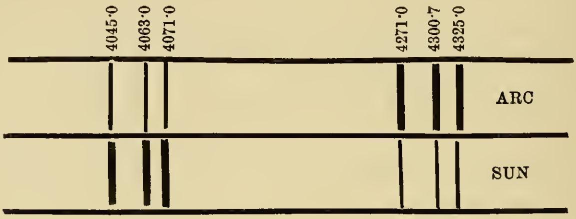

FIG. 109.—Anomalous reversals (iron).

solar storms, as already stated, the iron lines sometimes make their appearance in the chromosphere. Now, if we were dealing here with one molecular grouping, we should expect the lines to make their appearance in the order of their lengths, and we should expect the shortest lines to occur less frequently than the longest ones. But, precisely the opposite is the fact. Professor C. A. Young records in his observation of the chromospheric lines, made at Sherman, that two very faint and short lines close to the triplet near G were observed thirty times, while one only of the lines of the triplet was seen twice.

The intensity of the lines then is greatly changed when we pass from one set of observations taken under one set of conditions, to another set taken under other conditions. It is not a mere question of dropping out the lines when we pass from the temperature of the arc to the temperature of the coil, but in addition there is a considerable intensification of certain of the lines visible under one or another condition.

Closely adjacent lines sometimes show very great variations; three lines at wave-lengths 4918, 4919·8, and 4923·2 afford a case in point.

In the solar spectrum 4919·8 is thickest, in the oxhydrogen flame neither is visible, in the electric spark 4923·2 is thickest, while it is almost invisible in the electric arc; under no conditions are all intensified at once; each one seems intensified at the expense of the other.

In photographs of long reaches of the spectrum in which the results of the spark and arc temperatures are shown side by side, this wonderful variation comes out in its strongest form. This does not depend upon my own work only, for Messrs. Liveing and Dewar have recently published such photographs.[1]

We shall have to return to these lines afterwards.

The strongest contrasts of intensity are seen when the greater differences of temperature are employed; thus the greater differences are seen between the spectra of the oxyhydrogen flame and the jar spark.

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[1] *Phil. Trans.*, vol. 174, Part I., p. 212.

The line 5433 is seen rather faint in the sun and very strong in the oxyhydrogen flame. 5197·5 is very faint in the sun, but its intensity is doubled and even trebled with certain conditions of the coil.

I have introduced these facts in this place to repeat a remark about Kirchhoff's statement, which one is bound to insist upon in connection with this modern work. When Kirchhoff made his statement he was amply justified by the science of the time. He was familiar naturally with the spectrum of iron, which he had studied in his own laboratory, and other good observations of the spectrum of iron had also been recorded. But, with observations like these before one, which must be taken into account, it is too *coarse* a statement—I do not use the word in any offensive sense—to say that the iron lines in the sun correspond with the iron lines seen on the earth. Which iron lines—which of these horizons—is to be taken? It will be seen in a moment, if there are differences between these horizons, that if we take any one, we throw all the others out of court; and we have no right to do that. The statement about the coincidence in the intensity of solar and terrestrial spectra could not be made with the facts at our disposal now.

3. *Some Details of the Inversions of Iron Lines in Spot Spectra.*

The observations made of the various inversions of iron lines in sun-spots have been laid down on specially prepared maps, showing the results of the laboratory work, each strip of the map called an "horizon" showing the spectrum produced at each temperature.

In different horizons we have recorded the results observed when we use either the arc, or coil, or oxyhydrogen flame, and we vary in each case, as far as can be, the temperature employed.

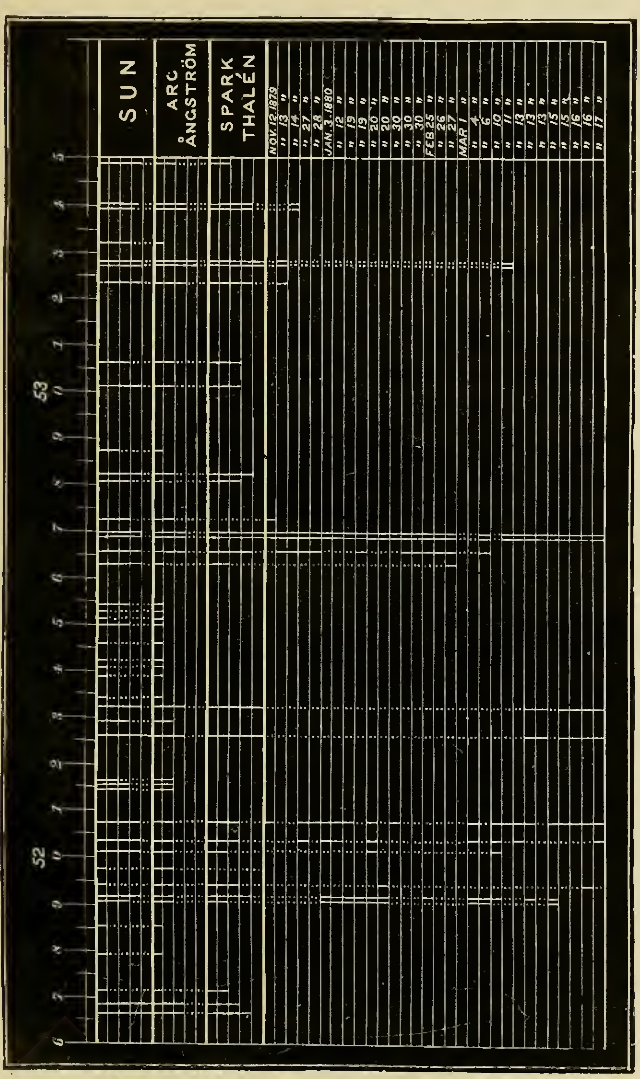


FIG. 110.—Portion of a large map, showing how the iron lines most affected in sun-spots are recorded. Region *b*—D The dates of observation are given at the side. The full lines show the lines widened in each spot day.

For instance, when we use the quantity coil we use a large jar, a little jar, or no jar at all; and the same with the intensity coil.

The ordinary solar spectrum is always included as one of the horizons. Thus in the case of iron all the iron lines were singled out from the Fraunhofer lines and were given separately, each with its true intensity.

The diagrams given are small portions of these maps—which were all on a large scale—prepared for each 100 observations of spots and for each region examined. They will show the method of record and comparison adopted.

In the horizon at the top marked "Sun," we have the iron lines recorded among the Fraunhofer lines. Below we have the lines given as iron lines by Ångström, who used an electric arc; while lower down we have the iron lines recorded by Thalén, who used the electric spark. It will be seen that there is a very considerable difference in the spectrum of iron as obtained by means of the spark and by means of the arc, and that there is an equal difference between the spectrum of iron in the sun, that is to say, in the total solar absorption registered by the Fraunhofer lines, and the spectrum of either the arc or the spark. It is also to be noted that the solar spectrum is more like the spectrum of the arc than the spectrum of the spark.

The results in the case of the spots are mapped as follows. We have in the vertical lines a record of the lines which are affected in each spot, and each of the spaces included between the horizontal lines represents the lines most widened in a particular spot, the date being given on the right-hand side.

Now the wonderful thing that one is at once struck with in all the iron maps alike, is the absolute and complete irregularity of the whole result. There is no continuity among any of these lines. A careful inspection of the maps shows us that, speaking in a general way, each of the lines is seen in one spot or another absolutely without the other. We have an *inversion* in the appearance of the lines when passing from spot to spot.

Further, whenever we get a line intensified by Thalén, we miss it in the spots, and, as a rule, what happens is that the

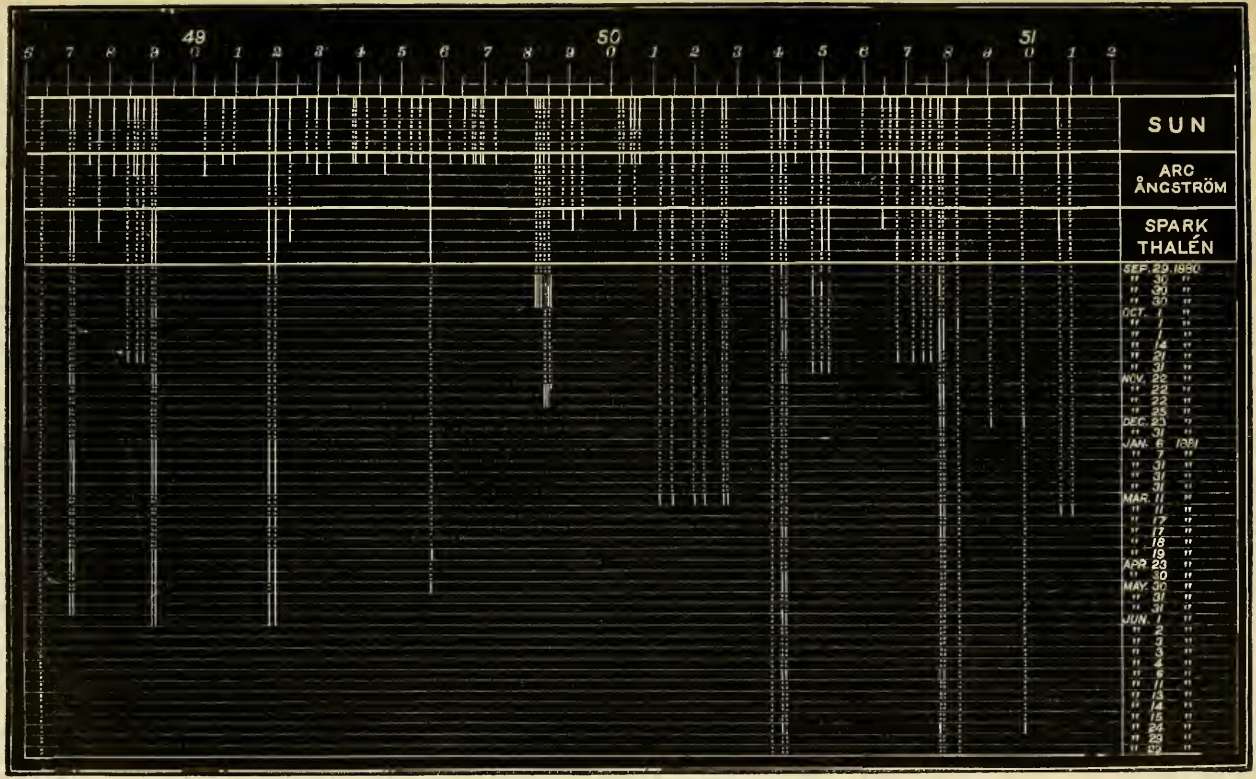


Fig. 111.—Portion of a large map showing how the iron lines most affected are recorded. Region F—*b*. The dates of observation are given at the side.

spectrum of the spot is not only simpler than the spectrum of the arc, but simpler than the spectrum of the spark. We shall call attention to other points later on.

4. *Comparison of Iron Lines in Spots and Prominences.*

Now the importance of these statements depends on other statements which we can bring to confront with them relating to other phenomena—in the present case the solar prominences.

To facilitate this confronting, what was next done was to mass the observations in hundreds, the individual observations of the sun-spots enabling us to plot out on another map the number of times each line was seen in a hundred observations. This number then gives us in the case of each line, its "Frequency in Sun-spots." The individual observations of prominences can naturally be treated in the same manner.

Next these individual observations both of spots and flames are treated in a certain way with reference to the discussion. I will explain what that certain way is: we have treated the sun-spot observations so that the lengths of the lines will represent the number of times they have been seen in 100 sun-spots; the line at wave-length 4919·5, for instance, has been seen seventy-two times; *that* line, in fact, has been seen more than any other; the one 5005·0 some forty times, and so on; very many lines having been seen less than ten times.

It is now the turn of the prominences, and the individual results obtained from Tacchini's observations of prominences have been summarised in exactly the same way. The next diagram shows the spot observations compared with those of 100 prominences seen between the years 1872 and 1876. The line 5017·5 was seen in 66 prominences out of 100. It is known that prominences exist in a region of the solar atmosphere not very far from that occupied by the spots, but we have already seen that whereas the spots are produced by a downrush of cool material, prominences are produced by an uprush of hot material. Let us see therefore if any change is produced in the phenomena; whether we shall have exactly the same lines from the flames or prominences as we have from the spots.

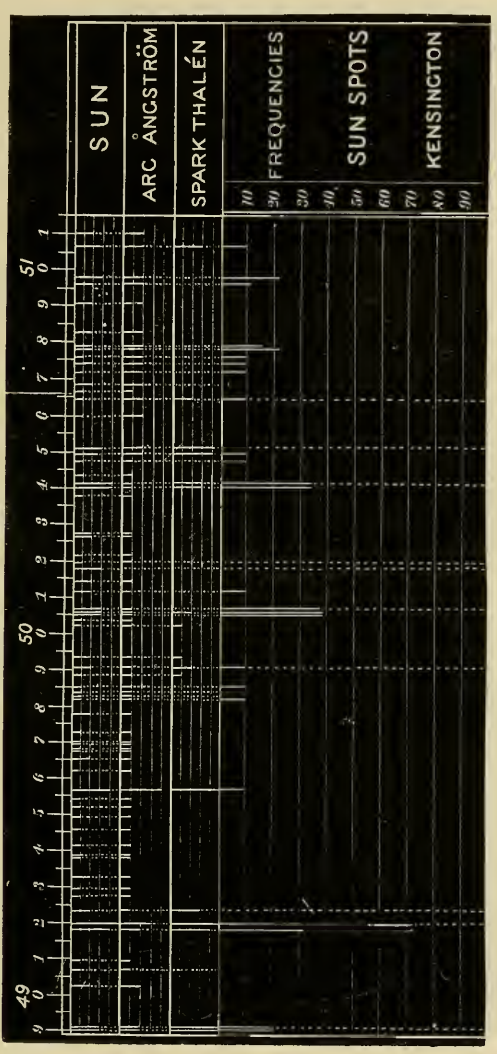


Fig. 112.—Frequencies of appearance of the iron lines in region F—b in 100 spots.

The facts with respect to Tacchini's observations are given in Fig. 113. We begin as before with the total absorption

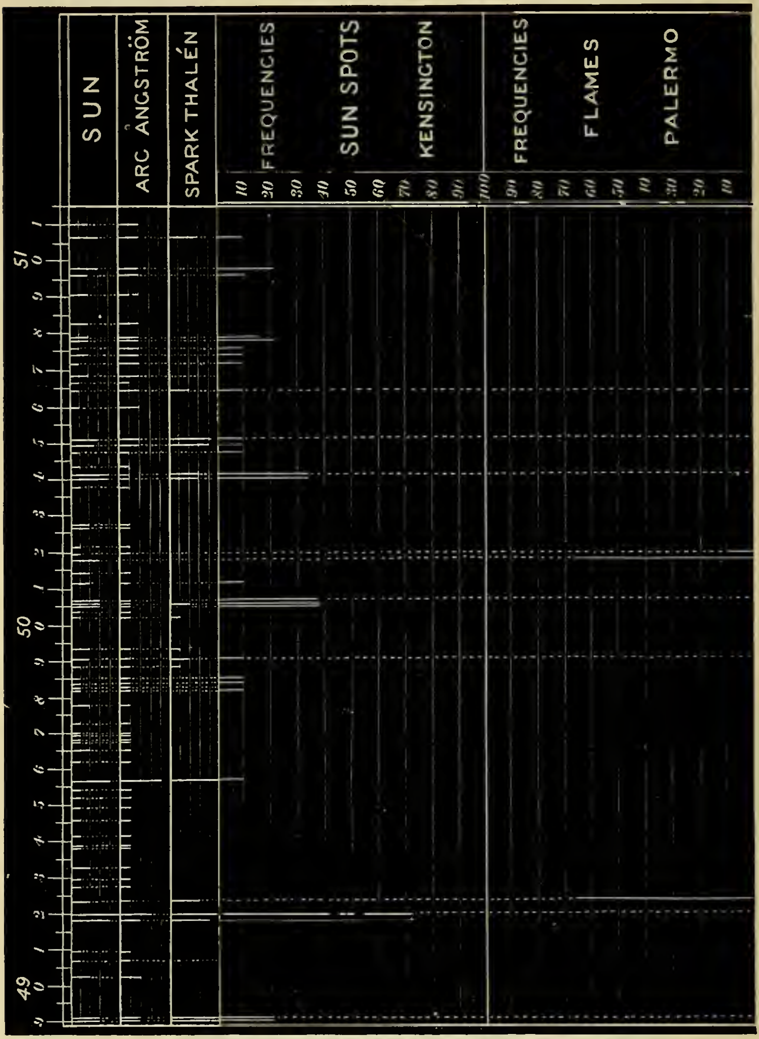


FIG. 113.—Iron spot lines seen at Kensington confronted with iron prominence lines seen at Palermo.

of the sun indicated by the iron Fraunhofer lines and the lines from Ångström's map, and Thalén's map.

To make the matter quite clear, the sun-spot frequencies are reproduced. In the next horizon are entered the frequencies with which iron lines were seen in observations of a hundred flames by Tacchini.

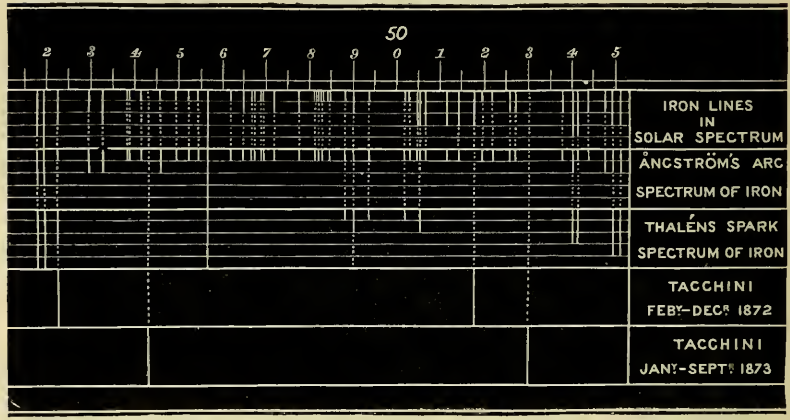
I am anxious to give these diagrams because they bring out the perfectly natural fact—for it is the natural fact—that over this region of the spectrum, at all events, no iron lines affected in the spots are visible in the prominences as a rule. If we were unacquainted with the spectrum of iron, we should be justified in saying that the "iron" spectrum of the prominences was due to one substance, and that of the spots to another.

If we assume, and we know this assumption is legitimate, that the region occupied by prominences is hotter than that occupied by spots, that hotter region ought to do its work, and it ought to be a work of simplification. Therefore I say it is a perfectly natural result, and not one to be wondered at, that in the spectra of the flames there is no line coincident with any of the lines seen frequently widened in the spots.

We have finally three solar spectra which we can compare one with the other. First of all we have the iron spectrum of the sun taken as a whole. Next we have the spectrum of spots, which we know to be hotter that the sun's atmosphere, taken as a whole. Then we have the spectrum of flames, which we know to be hotter than the spots. It will be seen that the story, as it runs from the top of the diagram downwards, is a story of greater simplicity, as it ought to be; the simplicity brought about by the reduction of lines actually seen as to number is accompanied by the appearance of new lines (produced by the transcendental temperatures) in these regions.

It will be observed that the line at 5017·5 given among the flame observations in the map is not recorded as an iron line by either Ångström or Thalén. The way, however, in which it sympathised with the line at 4923 in the flames induced me to look for it in iron with the intensity coil. I at once found it, and it is sympathetic with the line at 4923 in the spark as it is in the flames themselves. Though omitted by Thalén (most likely accidentally, though it may well be that he did not use sufficient tension to bring it out strongly), I have since found that it is recorded as an iron line by other observers.

Work in a laboratory at Kensington suggested that the observations in the Observatory of the Collegio Romano at Rome indicated that an omission had been made in a table of lines constructed at Upsala!


50
2
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IRON LINES IN SOLAR SPECTRUM
ÅNGSTRÖM'S ARC SPECTRUM OF IRON
THALÉNS SPARK SPECTRUM OF IRON
TACCHINI FEBY-DECR 1872
TACCHINI JANY-SEPTR 1873

Fig. 114.—Tacchini's observations of new prominence lines when the iron lines disappeared.

There is one very beautiful case that comes out from Tacchini's observations. From the beginning of February, 1872, Tacchini had observed the two iron lines 4923 and 5017·5, when suddenly the whole rhythm of his observation was broken, and at the end of December, 1872, these iron lines ceased to be visible in the flames altogether.

On no one occasion after this for some time was either of these iron lines observable, but from January to September, 1873, he saw two lines of wave-lengths, 4943 and 5031, about which absolutely nothing whatever is known; so that it really is, I think, a perfectly justifiable suggestion that these lines are the spectrum of a substance which exists in the chromosphere, which is produced at a much higher temperature than that needed to give us those other forms of "iron" which produce the lines in the spots.

That is a suggestion which is obvious from a reference to the maps, and if it is correct we must acknowledge that when the sun was in that intense state of quiescence that there were no violent descents—nothing to bring cooler vapours from the higher regions of the sun down to obstruct the general tenour of the solar way in the flame region, that at last, in consequence of this wonderful tranquillity, even the iron lines brightened in prominences—the only two lines which indicated the presence of iron in them—faded away as the iron lines faded from the spots, because iron, whether as we know it or not, faded away. There is no other explanation that I know of. In addition to those two lines we have two other lines about which we know nothing, except that they are probably due to a temperature which we cannot approach.

We are now in a position to sum up the spectroscopic evidence touching the relation between the spots and prominences. This may be included in the following:

General Statements regarding Spots and Prominences.

1. The chromospheric and prominence spectrum of any one substance, except in the case of hydrogen, is unlike the ordinary spectrum of the substance. For instance, we get two lines of iron out of 460. Thus we see that the spectrum of a substance in the prominences is very unlike its spectrum out of a prominence, that is, in our laboratories.

2. There are inversions of lines in the same elements in the prominences as there are inversions in the spots, that is to say, in certain prominences we see certain lines of a substance without others; in certain other prominences we see the other lines without the first ones.

3. Very few lines are strongly affected at once, as a rule, and a very small proportion altogether; smaller than in the case of spots.

4. The prominences are not so subject as spots to sudden changes so far as lines of the same element are concerned.

5. There is a change in the lines affected according to the sun-spot period.

6. The lines of a substance seen in the prominences are those which in our laboratories are observed to be considerably brightened when we change the arc spectrum for the spark spectrum.

7. None of the iron lines ordinarily visible in prominences are seen at the temperature of the oxy-hydrogen flame. Some of the oxy-hydrogen flame-lines are seen in the spots, but, as said before, none of these lines have ever been seen in the prominences.

8. A large number of lines ordinarily seen are of unknown origin.

9. Many of the lines seen are not ordinarily seen amongst the Fraunhofer lines. Some are bright lines.

10. As in the spots we found that the H and K lines of calcium in the ultra-violet were always bright in the spot-spectrum, the other lines of calcium and the other substances being darkened and widened, so also it would appear that the lines H and K of calcium are always bright in the prominences in which the other lines of calcium are generally unaffected.

11. Many of the lines are common to two or more elements with the dispersion which has been employed.

5. *The Test supplied by Change of Refrangibility.*

We have then got so far. Limiting our studies to iron we find that the prominence spectrum is made up of one set of lines seen in the terrestrial spectrum, and the spot spectrum is made up of another set. And more than this, if we add the lines seen in the prominence and spot spectra together we do not then by any means make up the complete spectrum.

Reference has already been made to another means of establishing this extraordinary fact of the separation of the iron lines in spots and storms —the change of refrangibility of the lines brought about by the change of velocity of movement of the various solar vapours. If, as already hinted, the lines of iron behave to each other in precisely the same way as the lines of two perfectly distinct substances behave to each other; then if we observe changes of refrangibility in the iron lines, both in spots and flames, we should get the same differentiation as we have already got in the lines thickened or intensified in the spectra of spots and flames.

For such observations as these it is absolutely imperative that the variations in the refrangibility of the lines, if they exist, should be seen in the same field of view at the same time. There must not be the least possibility of suggesting that the variations are due to the observations being made on different portions of the sun.

We will now see the results which have been obtained along this line of research, and it should be pointed out that it is not a method by which it is easy in a short time to accumulate a large number of observations, seeing that in the case of spots we not only want a spot, but we want that spot to be in a very considerable state of commotion, in order that the change of refrangibility may be obvious enough to enable us to record the phenomena.

One word as to prominences.

Some two or three years ago, when the sun-spot work revealed the different behaviour in different spots of lines visible in the spectra of the same element, it seemed desirable to extend similar observations to metallic prominences, and, if possible, in such a way that comparisons over a considerable reach of spectrum should be possible.

It then struck me that a grating cut in half, with one part movable, would afford a ready means of doing this. Circumstances prevented the realisation of this scheme till recently, when I put into the optician's hands a grating presented to me by Mr. Rutherfurd.

The result is excellent. It is possible to observe C and F, for instance, together, quite conveniently, with either a normal or a tangential slit. The only precautions necessary are to see that half of the light passing through the object-glass falls on the half grating, and that the rays which come to a focus on the slit plate are those the wave-lengths of which are half way between the wave-lengths of the two lines compared.

It is to be regretted that the method will not work so well with spots. The mixture of lines is too great, as two superimposed parts of spectra, both full of Fraunhofer lines, are then in question.

So far as this inquiry has gone at present we have observed the lines contorted in spots only in the case of lines seen in the same field of view at the same time.

In the diagram (Fig. 115) the zigzag lines indicate the iron lines which changed their refrangibility in a number of spots observed at the end of 1880. The point is that, although we have a great many of the iron lines bent, twisted, contorted—with their refrangibility changed, yet some of the iron lines mixed with them give us no indication of movement. In the diagram we have at 5366-70, three lines, two in motion and one at rest, all belonging to a well-known group of iron lines. At a later date we have the line at 5382 at rest, while that at 5378 is in motion.

These observations, then, prove there is just as much individuality in the way in which the lines of iron change their refrangibility as there is in the way in which one particular line, and then another, is thickened in a sun-spot or brightened in a prominence; and if we go further we find this very interesting and additional fact, that the lines which are not contorted are in a great many cases precisely those lines which are seen in the prominences, but not in the spots.

It is seen, therefore, that the evidence afforded by change of refrangibility is of like nature to that afforded by the thickening of lines in spots and brightening of lines in flames.

The explanation which lies on the surface is that the vapours in the flames produce one set of lines in one place or at a certain temperature, and the vapours in the spots produce another. Sometimes these vapours are mixed up by up-or-down rushes, and sometimes, therefore, the lines are common.

The diagram (Fig. 116) gives the main results in a convenient manner. It does not profess to go over the whole ground, but I think it will enable me to point out the way in which the phenomena observed on the sun are re-echoed and endorsed by the work which has been done in the laboratory, and how severe the tests applied have been, and how well the view has borne the strain. I have included in the diagram all the variations of intensity referred to in the present chapter concerning the lines.

The diagram refers to three iron lines visible in the map given on page 339—three lines that in an instrument of ordinary dispersion might easily be mistaken for a single line in the solar spectrum. We have, as before, the intensities among the Fraunhofer lines recorded in the upper part of the diagram; we then go to the photographs of the arc, and find that the line at 4923·2 is entirely absent. We then pass on to the quantity coil, which gives us the three lines; but there is a difference between the intensities of the lines as seen in the quantity coil with a jar, and the solar lines 4918 being thinner than in the solar spectrum. If we take the jar out of circuit 4923·2 almost disappears, and we get nearly the same result as from the arc.

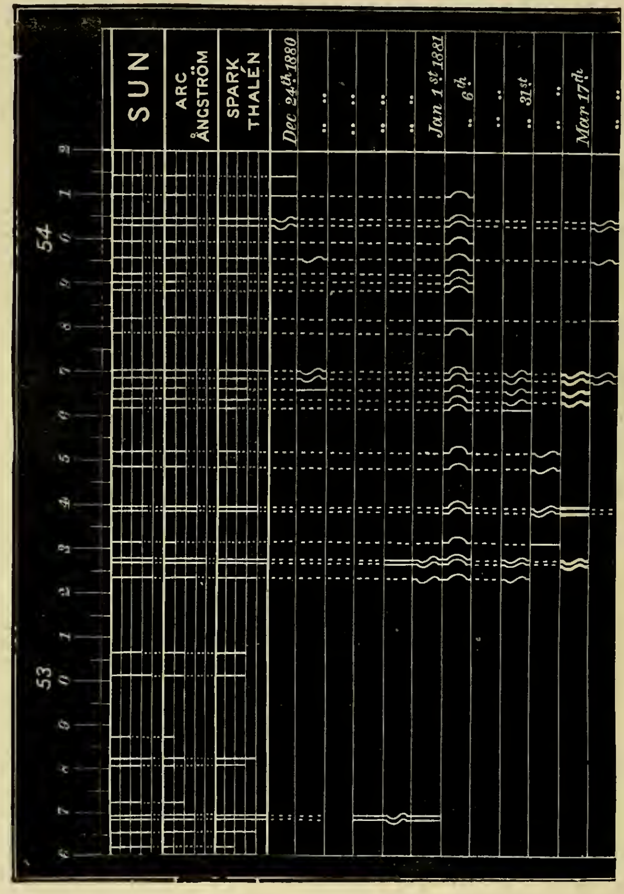

FIG. 115.—Different rates of motion registered by different iron lines.

We then try the intensity coil, which is supposed to give us an equivalent of higher temperature than the quantity coil does. What do we find then? That 4923·2 is enormously expanded, and developed apparently at the expense of 4918, which becomes thin. Taking the jar out, we come back to a result which is very much like the solar spectrum, with the difference, however, that 4918 is somewhat less intense.

Then come the facts which have already been brought forward (*antè* page 335) with special reference to these particular lines, that the two which are seen alone in the arc are seen alone in the spots, or at all events in 73 spots out of 100; and the other line which is so enormously expanded when we use the highest temperature is seen alone in 52 out of 100 prominences by Tacchini. Then we finally learn, that in several cases when a change of refrangibility has been observed in the iron lines in the spots visible on the sun, that the two lines 4918 and 4919·8 have been affected, while 4923·2 has remained at rest.

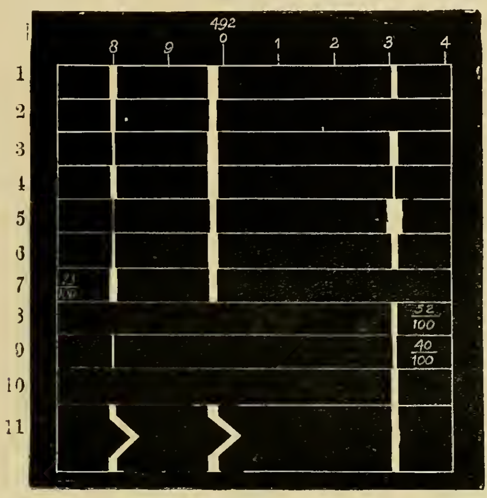

FIG. 116.—Diagram showing the behaviour of three iron lines under different conditions, solar and terrestrial. 1, solar spectrum ; 2, arc ; 3, quantity coil with jar ; 4, quantity coil without jar ; 5, intensity coil with jar ; 6, intensity coil without jar ; 7, spots observed at Kensington ; 8, prominences observed by Tacchini ; 9, prominences observed by Young ; 10, reversed in penumbra of spot observed on August 5, 1872, by Young ; 11, motion indicated by change of refrangibility.

That will give an idea of the way in which we really do find the laboratory work and the observatory work each coming to the rescue of the other, each helping us to understand something which, without the other record, would be excessively difficult; and how the results, strange and apparently anomalous according to the old view, obtained with regard to the thickness of the lines are exactly matched by equally anomalous results when we consider the changes of refrangibility.

Professor Young in referring to this part of the work writes as follows: —[1]

> "In the motion-distortions of lines Lockyer finds strong confirmation of his ideas. It not unfrequently happens that in the neighbourhood of a spot certain of the lines which we recognise as belonging to the spectrum of iron give evidence of violent motion, while close to them, other lines, equally characteristic of the laboratory spectrum of iron, show no disturbance at all. If we admit that what we call the spectrum of iron is really formed in our experiments by the super-position of two or more spectra belonging to its constituents, and that on the sun these constituents are for the most part restricted to different regions of widely varying pressure, temperature and elevation, it becomes easy to see how one set of the lines may be affected without the other. The same facts are of course also explicable on the supposition that there are several allotropic forms of iron-vapour, mixed together in terrestrial experiments but separated on the sun, and sorted out, so to speak, by the conditions of temperature and pressure."

With regard to the latter part of the paragraph, I may add that if it had been a fact generally accepted by chemists, "that there are several allotropic forms of iron-vapour, mixed together in terrestrial experiments but separated on the sun," a great part of this book need never have been written.

Messrs. Liveing and Dewar are more bold, they deny that the prominence member of the triplet is iron at all.

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[1] *The Sun*, page 100.

"The line at wave-length 4923 which occurs so often in the chromosphere, according to Young and Tacchini, and is assumed to be due to iron, is so near to lines which come out in our crucibles on the introduction of other metals, that one cannot help feeling some doubt as to its absolute identification with the iron line."[1]

But even this does not remove the difficulty, indeed it rather increases it, for if the line in question is not an iron line, then over a large reach of spectrum the iron vapour in the hottest regions of the sun open to our inquiries, instead of giving us two lines only gives us one.

6. *Bright Lines on the Disc and in Sun-spots.*

I may conclude this chapter by referring to another class of observations in which the differentiation of the iron lines is equally complete. How complete it is with regard to calcium I have elsewhere stated in referring to photographs of sun-spot spectra. In these the H and K lines are almost always photographed as bright lines over the spectrum of the umbra, while the blue line of calcium, which is *the* line of calcium at the temperature of the arc, is absolutely unaffected. Similar phenomena are seen when the iron lines are observed bright on the disc; of adjacent lines of equal intensity one is taken and the other left; and further, contortions may be seen in one differing entirely from those seen in the other.

On this point I shall content myself by referring the reader to Professor Young's observations recorded in his admirable book on the sun.[2]

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[1] *Proc. Roy. Soc.* xxxiii. 432.

[2] *The Sun*, by Professor Young. International Scientific Series, page 210, Fig. 64.

CHAPTER XXV.

TESTS AFFORDED BY ECLIPSE OBSERVATIONS.

During an eclipse of the sun, the moon by gradually coming between us and that body shields our atmosphere from the brilliant light of the photosphere.

If the eclipse is to be a total one, this shielding effect will be more decided, and if the totality is a long one it will be most decided. The reason of course is, that, as under the last condition the angular diameter of the moon is greater than that of the sun, the air will be shielded, not only from the light of the photosphere, but even from that of the brighter layers of the superincumbent atmosphere.

Naturally after the middle of totality the amount of shielding will be gradually reduced, as it was gradually increased up to that instant.

Now the sun's atmosphere above the photosphere is masked ordinarily, as are the stars, by the illumination of our air due to the photospheric light, and more effectively because the particles in our air, the reflection by which produces this illumination, are in a position to reflect more light when they lie nearly between us and the sun than they are elsewhere.

When the moon, therefore, during an eclipse shields our atmosphere from this illumination, the effect is as if a series of veils was very gradually withdrawn; and the sun's atmosphere and the stars, first dimly and finally magnificently shine out in all their glory; till again as gradually as they were withdrawn, the veils fall.

I have said gradually, but that is really not the right word to use, because at certain instants the light changes suddenly. In order to study this more closely let us take the first half of the eclipse from the beginning to the middle of totality, in the other half of course any change indicated will occur in inverse order. From the time that the edge of the moon is first seen biting into the disc of the sun, until the sun is very nearly covered, the change is very gradual. When only a thin crescent of the sun is left the darkness increases rapidly; and there is a very sudden decrease in the illumination at the instant when the photosphere is finally covered. At this moment, in fact, the shadow of the moon is seen sweeping through the air almost like a solid thing and as if it could carry devastation in its course. This is one of the most terrible moments of the eclipse to the ignorant multitude. For a few seconds after this sudden darkening the light still decreases until the lower corona is hidden; and then, after another definite but less sudden reduction of illumination than the former one, we get the period of greatest obscuration.

Now the withdrawing of the veils only begins to be really effective some ten minutes before totality, and the effectiveness of this withdrawal is first indicated by the spectroscope. Lines too faint to be visible in the chromosphere in full sunlight are now seen for the first time, and the number increases as the withdrawing of the veils goes on. Passing from the spectroscope and dealing with ordinary observation, we may say that, five minutes before totality, if the eye has been shielded, the corona becomes visible, and its details can be faintly made out in a telescope.

At the instant of the disappearance of the sun the sudden decrease of light naturally affects all the observations with the naked eye, and in the telescope all the more delicate and beautiful features of the corona burst out in all their details. In the spectroscope the sudden withdrawal of the continuous spectrum, especially if a tangential slit be used, causes all the lines, the number of which has been visibly increasing for the last ten minutes as we have seen, to be seen alone on a dark background, although they are not intrinsically brighter than they were before. This sudden withdrawal of the luminous background gives rise at times to what has been called a flash. With a radial slit, at this moment the spectrum is fullest of lines for a reason which we shall refer to in the sequel, but the brightest quickly disappear, and the faintest lines which are the most numerous, attain a still greater visibility at the moment of the second sudden decrease to which we have referred, the instant, namely, at which the inner corona is eclipsed.

Such being the actual conditions of observation during an eclipse, let us now turn back to chapter xxii., in which the solar atmosphere on the new hypothesis is considered.

I have now to show in the first place that this gradual unveiling of that atmosphere enables us to apply a test of the utmost stringency. If we turn back to figure 104, on the new hypothesis stratum A will be hotter than B; B hotter than C, and so on; and different molecules exist in these different strata.

Now at this point a very important consideration comes in, for if the hypothesis be correct we can predict the locus of any particular spectral variation by considering temperature alone.

It was stated (p. 306) while discussing the conditions of observation, that whether we were dealing with strata of substances extending down to the sun or limited to certain heights, the spectral lines would always appear to rest on the solar spectrum, and that the phenomena would *in the main* be the same.

But though they would be the same in the main, there would be a difference, and this supplies us with a test between the rival hypotheses of the greatest stringency. The stratum B, being further removed from the photosphere than the stratum A, will be cooler, its lines therefore will be dimmer, and the lines of C will be dimmer than the lines of B, and so on. So if we could really observe the strata, *the longer a line is,* i.e., the greater the height at which the stratum which gives rise to it lies, *the dimmer the line will be.*

Our best chance of making such an observation as this is during a total eclipse. We do not, as we have already remarked, see the lines ordinarily in consequence of the illumination of our air. As during an eclipse before totality the intensity of this illumination is rapidly diminishing, the lines first visible should be short and bright, and should remain short, while the new lines which become visible as the darkness increases should be of gradually increasing length; so that the spectrum should become richer in the way indicated in fig. 117. And as the spectral lines due to the molecules existing in

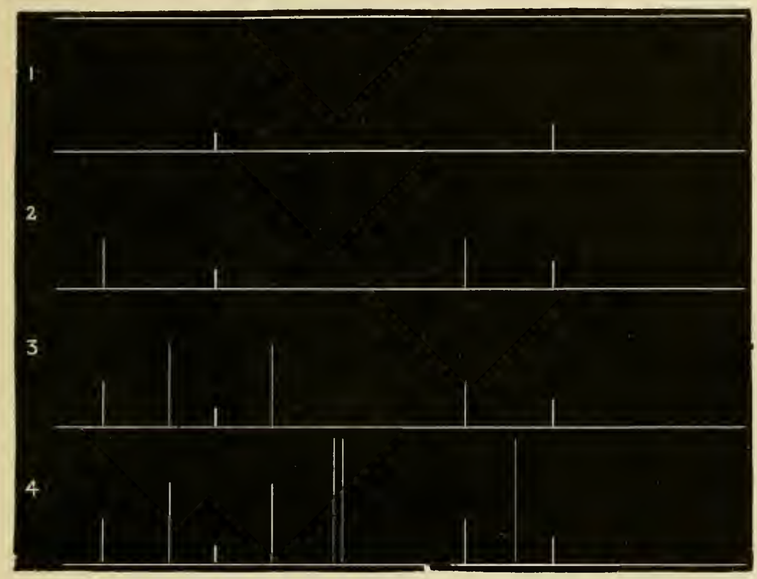

FIG. 117.—The shorter lines will be most intense on one hypothesis, and not on the other.

these higher strata are gradually added, the spectrum must end by becoming very rich indeed in lines of gradually increasing faintness and length.

The point here raised is so important that it will be well to dwell on some details. If the short lines seen at first are limited to a low stratum no reduction of the illumination will make them seem higher than they did at first. And speaking generally, all the lines existing in the lower stratum alone should be seen at first, because the molecules which give rise to them are at the same transcendental temperature. The difference, therefore, between the short lines seen at first and the longer dim lines gradually revealed—each degree of unveiling being accompanied by the appearance of a set of longer lines—will become more marked as totality is approached. We cannot make these observations by the ordinary method so well as in an eclipse, because the lines are brightest when the conditions are most disturbed, and when we cannot tell, for reasons I shall show afterwards, whether we are dealing with ascending or descending currents, or both combined; but during the approach to totality, and naturally, therefore, the recession after it, we can watch the phenomena on an undisturbed part of the sun through a long range of varying illumination.

This is the place to point out that the new hypothesis has to explain everything by appealing to changes of temperature alone, and that if such a consideration of temperature fails to accord it a power of prediction, the hypothesis must fall to the ground. Others who have written on this subject have suggested that the various phenomena which they cannot explain may be dependent upon variations of pressure and density, about which at the sun at present we know nothing or next to nothing, or upon other more obscure conditions which are not even defined. It is in this sense that I have said that success here is crucial for the hypothesis.

The test was first applied during the eclipse which occurred in 1882 in Egypt, and afterwards, in 1886, in the island of Grenada in the West Indies. Nor was this all; the lines to be first seen as short and brilliant, and the lines to be seen later as long and dim, were predicted a year before.

In May, 1881, in an address delivered to the Astronomical Society I pointed out the importance of observing the eclipse of 1882. The following part of the address is all I need give here:—

"There is an eclipse of the sun next year, lasting only, I am sorry to say, a minute and a very few seconds; but there is to be another the year after, lasting nearly six minutes, but it happens to be in a part of the world where it is always afternoon. In the observations of the future we must pay attention to these lines which have been picked out by Nature herself in the spots and prominences. If I observed either of these eclipses, I should be content to fix my instrument on these three iron lines between 4,900 and 5,000 ten millionths of a millimetre, because, of these three lines which are in the Fraunhofer spectrum, two have always been seen in spots without the third, and the third has always been seen in the prominences without the other two. If, then, the spectrum of the flames represents the lowest part of the atmosphere, and the spectrum of the spots represents the atmosphere above the flames and below the corona, then we ought to see these lines different in the corona, and in the corona we ought to see the lines which are dropped in these two regions. Of the twelve lines between 4,900 and 4,957, only one is picked out by Thalèn for intensification, and that particular line is the line seen alone in the region of the prominences. There are eleven lines which are absolutely untouched by Thalèn, showing that absorption must be proceeding somewhere, and it is most interesting to determine where it is going on.

"In the Indian eclipse, in 1871, I saw lines reversed before totality. I saw, as it were, hundreds of lines; but if I had confined my attention to these three lines I should have got a better idea of what the magnificent flashing out of those lines meant. It has been called the reversing layer; but I do not now believe it is the reversing layer for a moment, for, when it comes to be examined, we shall probably find that scarcely any of the Fraunhofer lines owe their origin to it, and we shall have a spectrum which is not a counterpart of the solar spectrum."

I followed up this address to the Astronomical Society by the following memorandum, which I laid before the Solar Physics Committee :—

"The total eclipse of the sun which takes place in May next year will be visible in such an accessible region, that it is to be hoped that the precedents of 1860, 1870, 1871, and 1875, will be followed and steps taken to secure observations, the more especially as the eclipse will happen somewhat near to the period of maximum sunspots, and will allow of a comparison being made with the results obtained in India in 1871.

"There is one new point (it is not necessary now to refer to the importance of registering the ordinary phenomena) to which I beg to invite the attention of the Committee.

"The discussion of the sun-spot spectra recently observed at Kensington, and of the prominence spectra observed at Palermo by Tacchini, since 1872, throws some doubt upon the validity of some of the conclusions based upon the results obtained by the English and American Government Eclipse Expeditions in 1870.

"In that year, at the moment of the disappearance of the sun, a large number of bright lines was seen to flash out, and it was supposed that these lines composed the spectrum of a thin layer near the sun, and were those the reversal of which produced the lines of Fraunhofer.

"Hence this layer has been termed, and generally accepted to be, the reversing layer. The conclusion seemed to be in harmony with the results obtained by Dr. Frankland and myself, who gave reasons for showing that the region in which the absorption of the elementary bodies of greater atomic weight than hydrogen, magnesium, and sodium, must be below the chromosphere. This view was put forward at a time when the elementary nature of the so-called elements was never questioned and before any of the recent results had been obtained.

"The observations made by the Government Eclipse Expedition which went to India in 1871, showed that this flashing out of lines was a real phenomenon; but as the observation was a general one, and as, during the eclipse, the Fraunhofer lines were invisible, there was no absolute demonstration of the identity of the two spectra.

"The facts now beyond question, that *quâ* the same element, the spectra of spots and flames differ, and that the spectra differ widely among themselves, throw great doubt upon the conclusion to which reference has been made.

"First they seem to indicate that some of the absorption takes place at a higher level than that occupied by the so-called reversing layer.

"Secondly, they seem to indicate that many of the brightest lines seen during the flash to which reference has been made may be those seen thickened in spots or intensified in the prominences, although they do not occur except as excessively faint lines among the Fraunhofer lines.

"In short, in 1870, the fact that the spot and prominence spectra are so widely different from the ordinary solar spectrum, had not received the attention it must receive in the light of the most recent inquiries, and it was taken for granted that because a large number of lines was seen, that, therefore, they occupied the same positions as the large number of lines which compose the ordinary solar spectrum.

"The recent work seems to show that the complete absorption spectrum of any one element is produced, not at one level, but at various levels, the absorption of all the levels being added together to give us the complete result.

"If this be so, the lines seen in the flash will not be Fraunhoferic lines with the ordinary intensities."

It follows then, from the considerations thus set forth, that the lines seen best before or after totality, or brightest and shortest in the flash at the beginning or end of totality, should be those seen in prominences, and not in spots, and relatively brighter in the spark than in the arc; while the longer lines should be those lines affected in spots, and *not* in prominences, and should not be brightened on passing from the arc to the spark.

The actual observations of the lines made at Sohag are shown in the accompanying map, and these eclipse observations are compared with the lines thickened in spots, the lines observed in the prominences by Tacchini and those intensified on passing from the arc to the spark. The Fraunhofer lines are also given according to Ångström, and the iron spectrum of the arc and spark according to Ångström and Thalèn—the latter corrected.

The observations during the eclipse were made seven minutes, three minutes, and two minutes before totality, as the air was gradually darkened; successive veils, as it were, being gradually lifted, as I have explained, so that the more delicate phenomena could be successively seen.

Dealing with the iron spectrum we begin (seven minutes before totality) with one short and brilliant line constantly seen in prominences, never seen in spots. Next, another line appears, also short and brilliant, constantly seen in prominences. And now, for the first time, a longer and thinner line appears, occasionally noted as widened in spots: while last of all we get very long, very delicate relatively, two lines almost constantly seen widened in spots, and another line not seen in the spark and never yet recorded as widened in the spots.

The procession from the hot to the colder is apparent, and the simplicity of the spectrum as opposed to the Fraunhofer spectrum even yet, is eloquent of the gradual approximation which would be still possible if the darkness could be greater and our attack more complete.

It will be noted over what a small range of spectrum the observations extend. We want similar observations over a wider range during future eclipses, and to do this work properly many observers armed with similar instruments must divide the whole or part of the solar spectrum amongst them,

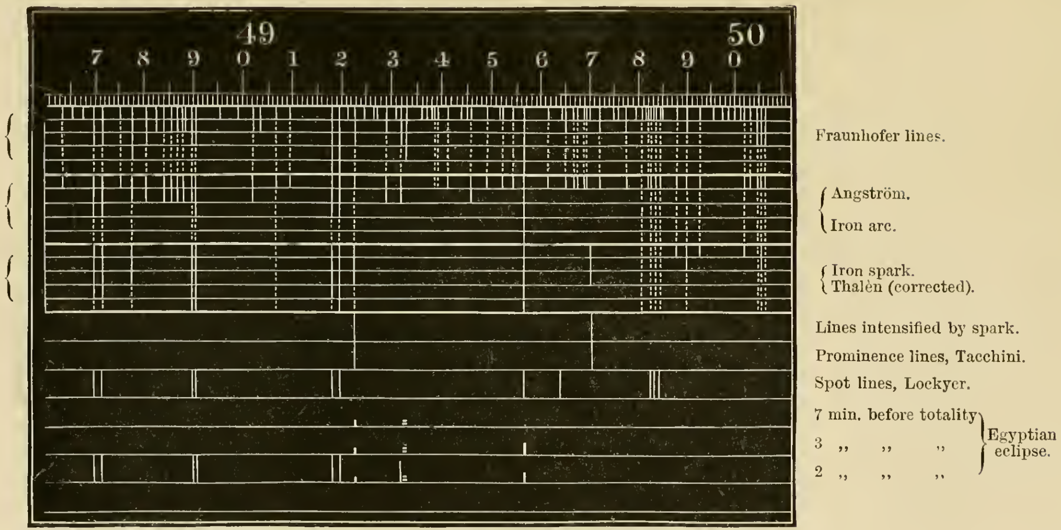

FIG. 118.—THE LINES SEEN IN THE EGYPTIAN OBSERVATIONS.

The barium line at λ 4933·4 is a line seen thirty times by Young at the maximum spot period, and not recorded by Tacchini at the minimum. The lower longer line not seen till five minutes afterwards is a titanium line λ 4932·5.

preferably that part between F and D, which has been most closely watched in prominences and spots by Tacchini and myself.

The observations in 1886 were made by Mr. Turner, chief-assistant at the Greenwich Observatory, and although they are not yet published *in extenso*, it is known that they confirm in the main the Egyptian results. Nearly the same lines were seen as in Egypt in 1882, short and bright and long and thin.

It will be clear, then, that the new hypothesis has borne this test in a very satisfactory manner.

Hence a consideration of variations of temperature alone is sufficient to explain all the phenomena observed. We do not want to introduce an appeal to varying pressures and densities, and other molecular behaviour about which, at the sun, we practically know nothing, and about which, therefore, we may assume anything we choose, while it is certain that no controlling experiments are available. This is not a good basis on which to build, or from which to attack, any hypothesis.

I next pass to another point, concerning which eclipses, and eclipses alone, enable us to bring yet another test into the field.

In Fig. 104 we considered the sun's atmosphere, taking the simplest case, that of one element; when the sun cools it will be a very complex mass chemically. If the laws of evolution hold we need not expect that this will largely increase the complexity of the hottest layers A and B, but higher up, say at H—L, the complexity of chemical forms produced by evolution along the fittest lines will be very considerable.

These strata H—L may be taken to represent the corona. Its spectrum, therefore, should not be a continuous one, but should consist of an integration of all the radiations and absorptions of these excessively complex layers.

The earliest spectroscopic observations of the corona recorded it as giving us a continuous spectrum such as is given by a solid body, say the limelight, or the carbon pole in an electric lamp. Associated with the continuous spectrum were certain bright lines, so that astronomers were justified in regarding it as built up of solid or liquid particles floating in certain gases permanent at the temperature of the solar atmosphere.

This exactly fitted the old view, which supposed that all the vapours which produce the Fraunhofer lines existed close to the photosphere, and that there alone was there sufficient temperature to give us a line spectrum.

It was natural, therefore, that after the eclipse of 1870, during which it was supposed at the time, by myself among others, that such a rich harvest of facts had been obtained confirming the old view, that the detection of light reflected by these solid or liquid particles was attempted. That this power of reflection existed was found by Janssen in 1871, who saw the D line dark in the spectrum of the corona.

When I was first driven to the views I now hold, it became obvious that if these statements regarding the corona were strictly accurate my hypothesis was worthless.

I was not able to make any crucial observations during the eclipse of 1878 observed in the Rocky Mountains, for the reason that my instrumental equipment was of the lightest. But in 1882, when I had the opportunity of pursuing this inquiry, the result at which I and others arrived was of no uncertain sound. The spectrum of the corona as I saw it in Egypt was of the most complex nature. It was distinctly not a continuous spectrum resembling that given by the limelight. Instead of the gradual smooth toning seen, say in the spectrum of the limelight, there were maxima and minima producing an appearance of ribbed structure, the lines of hydrogen and 1474 being, of course, over all. Of other bright lines distinct at any great distance from the photosphere I saw none, nor any very marked absorption lines, not even D or *b*, but I should add that I naturally had no solar spectrum to guide me; but what I really did see was as if all the banded spectra I had ever seen were superposed. My instrument was a short focus photographic lens of 6-inch aperture and one flint prism; so that the light in the spectrum was ample. One could be certain of what one saw. I trust that at no distant day this observation will be confirmed with as powerful instrumental appliances.

But this evidence as to the very complex chemical nature of the corona does not rest upon my own observations alone, a photograph was secured of its spectrum in the blue and ultra violet on one of Captain Abney's dry plates, and it is stated that lines are seen in it which are special to the corona, as opposed to the prominences.

We are driven, then, to the conclusion that the corona is a mixture of incandescent vapours and solid or liquid particles, and if the hypothesis be granted we can make a stride forward towards simplification, and explain the phenomena presented by the corona by suggesting that *the vapours are due to dissociation at the photospheric level, and the particles which in part give the continuous spectrum to association at higher levels.* In this way the vertical currents in the solar atmosphere, both ascending and descending, the intense absorption in sun-spots, and their association with the faculæ, find an easy solution in connection with the spectrum of the corona, and its structure.

It is clear that the association to which I have referred is practically the production of solid masses of matter which if we could get at them would be found to resemble those other masses which we call meteorites. This is not the first time that the word meteorite has been used in connection with this spectrum of the corona, but I am not aware that it has anywhere been clearly pointed out that some solar meteorites, at all events, must be home-made. The suggestion that the phenomena of the corona are due to cosmical meteorites has been made over and over again.

That some meteorites fall upon the sun as upon our earth is undoubted, but with the solar conditions it is impossible that meteorites shall not be formed whenever the upward currents carry the associating metallic vapours to an elevated region where the temperature is low enough to make them condense into a solid form.

On this point I quote the following from a presidential address to the American Association recently given by Professor Newton, our highest authority in meteoretic matters.

> "A meteoroid origin has been assigned to the light of the solar corona. It is not unreasonable to suppose that the amount of the meteoroid matter should increase toward the sun, and that the illumination of such matter would be much greater near the solar surface. But it is difficult to explain upon such an hypothesis the radial structure, the rifts, and the shape of the curved lines, that are marked features of the corona. These seem to be inconsistent with any conceivable arrangement of meteoroids in the vicinity of the sun. If the meteoroids are arranged at random, there should be a uniform shading away of light as we go from the sun. If the meteoroids are in streams along cometary orbits, all lines bounding the light and shade in the coronal light should evidently be projections of conic sections of which the sun's centre is the focus. There are curved lines in abundance in the coronal light, but, as figured by observers and in the photographs, they seem to be entirely unlike such projections of conic sections. Only by a violent treatment of the observations can the curves be made to represent such projections. *They look as though they were due to forces at the sun's surface rather than at his centre.*[1] If those complicated lines have any meteoroid origin (which seems very unlikely), they suggest the phenomena of comets' tails rather than meteoroid streams or sporadic meteors. The hypothesis that the long rays of light which sometimes have been seen to extend several degrees from the sun at the time of the solar eclipse are meteor streams seen edgewise, seems possibly true, but not at all probable."[2]

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[1] The italics are mine.—J. N. L.

[2] *Nature*, Sept. 30th, 1866, page 532.

CHAPTER XXVI.

THE BASIC LINES.

It was pointed out in chapter xviii. that the arguments which I have advanced in favour of the dissociation of the chemical elements were based upon strict analogies. At the time my early papers were presented to the Royal Society we knew that in the case of mixed vapours the intensity of the lines of each constituent of the mixture changes as the relative proportions of the constituents change. We also knew that in compounds having a common constituent, such for instance as the salts of calcium, which have calcium common to them all, the lines of the metal are common to all the spectra when sufficient temperature is employed to decompose the salt.

Hence, arguing by analogy, short lines seen coincident in the spectra of two or more elementary bodies with the dispersion employed—a qualification which was never lost sight of—were easily explained on the hypothesis which suggested that the action which produced them was the same as that which had already been at work in the salts.

Had my paper been presented to the Royal Society some years later another series of facts would have had to be dealt with, for during those years facts have been rapidly accumulating which must have been taken into consideration in any general view.

Many observers have shown that in the case of various substances there is evidence to suggest that the refrangibility of lines is slightly changed by change of chemical composition or physical condition. My attention was first called to this by Professor Stokes, who had observed that one of the bands in chlorophyll changes its refrangibility when different solvents of the chlorophyll are employed.

Much more evidence of the same kind has in fact necessitated the introduction of the word "shifts" to define these slight changes of refrangibility, or want of coincidence, of apparently the same line under different chemical and physical conditions.

The following quotation from a paper by Herr W. Vogel supplies a reference to some of the work done on this subject :—

> "It is known that the position of the absorption-band of a substance depends very essentially on the dispersion of the medium in which it is dissolved or incorporated. One often observes that in strongly dispersive media the absorption-bands of a substance are displaced towards the red.[1] Now, the remarkable case often here occurs that certain absorption-bands are displaced with the increase of dispersion of the solvent, while others are not. Thus Hagenbach observed that, *e.g.*, the chlorophyll bands I. III. and IV. lie more towards red in alcoholic than in etheric solution, while the band II. in both solutions shows exactly the same position. I observed similar cases with uranium protoxide salts[2] and with cobalt compounds.[3]
>
> "Now Kundt has already called attention to the fact, that for absorption-spectra of gases the same rule holds good as for the absorption-spectra of liquid substances. He adds, indeed: 'It is only questionable whether, if, *e.g.* hyponitrate gas be mixed with various other transparent gases, the displacements of the absorption-bands are so considerable, that they can be perceived.' This doubt, however, does not affect the rule supposed, but merely its experimental verification. The supposition then is permissible that, in

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[1] Kundt, *Jubelband Pogg. Ann.* p. 620.

[2] Vogel, "Pract. Spect. analyse," p. 248.

[3] *Monatbr. der Akad. der Wiss.* of May 20, 1878.

the same way as with liquids, added media also affect the position of absorption-bands in the case of gases, and that in this case, as in the other, displacements of certain bands occur, while the position of others remains unaltered."[1]

Indeed the more this question of "shifts" is looked at the more important seem to be the consequences which may follow from its complete study. Lecoq de Boisbaudran and Ciamician may be cited among those who have pointed out that corresponding lines and bands in the spectra of the elementary bodies themselves may owe their origin to some such process.

If all the information now available had been before me in 1878 it would have been necessary to point out, that with regard to the position of lines due to the same substance, existing differently compounded, or under different physical conditions, there were *two* analogies open to us to throw light upon the possibility of common lines being detected in the chemical elements. The analogy of the salts of calcium above referred to, telling us that such lines would be coincident; the analogy as supplied by the shifts telling us that such lines would probably *not* be coincident, because they represent the vibrations of still finer molecules which might have been shifted on the formation of the element itself, and shifted differently probably in each element if the same molecule entered into the formation of several. If the evidence had been thus stated there would have been ground, I think, to accompany it with the suggestion that the shifts observed in the spectra of the elementary bodies would not be so great as in the case of the higher complexes; in fact that lines apparently coincident with small dispersion might be shown to be not truly coincident when higher dispersions were employed. It may be urged that this would render very doubtful any argument depending upon the basic-line part of the hypothesis. It certainly requires the statement touching basic lines to be modified, for now being in presence of two series of facts from which to draw analogies instead of one, it would have been necessary to point out that such lines if they existed might either be exactly coincident on the calcium analogy or nearly coincident on the shift analogy. But the modification after all is a small one. Instead of insisting upon the exact coincidence of lines in different substances, at one particular wave-length; a massing of the lines round that wave-length would have been suggested in addition. I am most anxious that this should not be regarded as special pleading or as an ingenious evasion of a difficulty. I am only stating now what I certainly should have stated in 1878, had the facts at present known been then available.

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[1] *Nature*, vol. xxvii. p. 233.

From the very first, as I have already stated, I have guarded myself carefully against the statement that the basic lines recorded by Thalèn and myself in my earlier work were anything more than coincidences with the dispersion employed. Indeed in 1881, in referring to the basic-line part of the hypothesis, I wrote as follows:—[1]

> "Here we must confess both our imperfect instrumental and mental means. We cannot talk of *absolute* coincidence because the next application of greater instrumental appliances may show a want of coincidence. On the other hand there may be reasons about which we know at present absolutely nothing which should make absolute coincidence impossible under the circumstances stated. The lines of the finer constituents of matter may be liable to the same process of shifting as that at work in compound bodies when the associated molecules are changed; but however this may be the fact remains, whatever the explanation may be, that the lines of the elementary bodies mass themselves in those parts of the spectrum occupied by the prominent lines in solar spots and storms."

We now undoubtedly know that with the higher dispersion which we can now employ the lines indicated by Thalèn and myself as common lines are not truly coincident. My assistants and myself, and others too, have gone over a large number of these lines. My own method was to use a concave grating by Rowland of eleven feet focus, kindly placed at my disposal by Captain Abney, and a powerful electric arc.

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[1] *Nature*, 1881, vol. xxiv. p. 370.

But while this work has been going on other work has entirely endorsed the previous conclusion that these nearly coincident lines certainly differ from other lines of the spectrum taken at random. This previous conclusion and the work which led to it have already been stated in chapter xviii. What I propose to do now is to refer to the later work in which these basic lines have again been noticed to behave differently from others. This difference of behaviour has been observed in the discussion of the observations of sun-spot spectra in connection with the spectra of prominences; and what we have to do is to see what has been the result of this inquiry with regard to the basic nature of some of the lines. Have we, as a matter of fact, or have we not, in these most widened lines in spots, and the most brightened lines in flames, picked out those lines which are common to two substances with the reservation alluded to above?

The facts will be gathered from the accompanying map of a limited region of the spectrum. We have under the data for the spectrum of iron formerly given in the first horizon the lines recorded by Ångström in his first memoir as common to two substances; the names of the two substances being given below. In the fourth horizon we have the observations of Thalèn made a few years after the observations of Ångström, and in passing from Ångström to Thalèn we pass from the temperature of the arc to the temperature of the induction coil. Now it will be seen that Thalèn also gives us lines in some cases agreeing with Ångström's, in other cases extending the information given by him, and in order to make this work as complete as possible I have gone over this region with the arc as Ångström did, and with the induction coil as Thalèn did, only I have had the advantage probably of using a more powerful coil. In fact I have used two coils—one so arranged as to give us the maximum effect of tension, and the other the maximum effect of quantity. In the first place it will be seen there is a general agreement between the observations—an agreement marred only in appearance here and there by the fact that in some cases the lines are so near the position of air lines that it has been impossible to make the observation absolutely complete. In other cases the appearance of imperfection arises from the cause that lines which are not seen at the temperature of the arc begin to make their appearance at the temperature of the coil; so that in a case like that at wave-length 5017, for

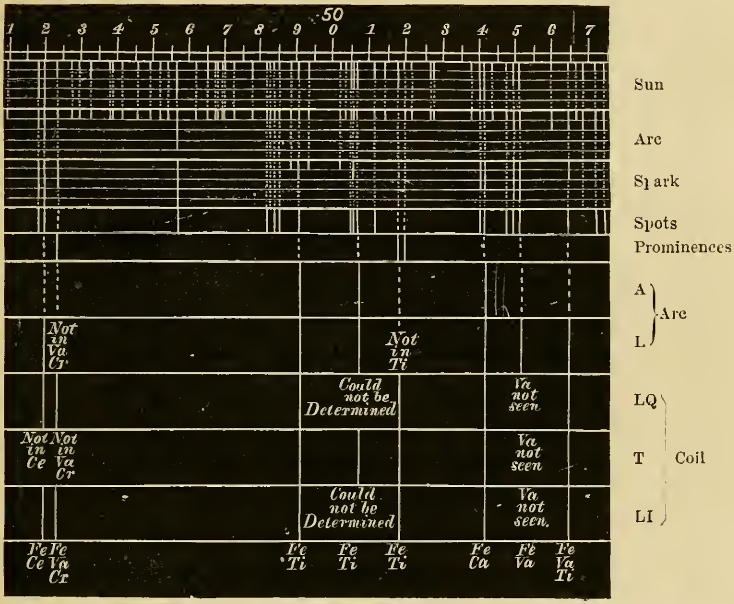

FIG. 119.—Showing that in the case of lines frequently seen in spots and prominences, lines in the spectra of two substances occur in those positions.

instance, where Ångström gives no line as common to two substances, yet Thalèn does. We find that both are right; that at the temperature of the electric arc that line does not appear in one substance or the other, while at the temperature both of the quantity and the intensity induction coils the line is certainly there. In Fig. 119 A represents Ångström's work, T Thalèn's, and L Q and L I my own work with the quantity and intensity coil.

What then is the final result? It is this; almost every important line in the spots, every important line in the storms, has been picked up by this method, and in fact *the map of basic lines along this region is practically a map of the lines widened in spots and present in storms, and nothing else.* Now it may be said that result is interesting, and perhaps important, but that it deals only with a very limited part of the inquiry. That is perfectly true.

The spectrum of iron and the spectra of other substances have however been attacked in other regions. It is unnecessary to go into more details here, but the general result may be expressed in rather a different way, and it will then be easy to see the extraordinary parallelism which goes on between two perfectly distinct sets of facts; first, the statement that such and such a line is seen in the spectra of two or more substances, and then the other statement that such and such a line is seen widened in spots or brightened in flames. I give below the numbers for the two regions which I have already discussed, the region from F to *b*, and from *b* towards D, in the case of the three metals—iron, nickel, and calcium.

Iron.

	F-*b*		*b*-548
Total number of lines	96	.	67
Number in spots and prominences	38	.	41
Basic lines	15	.	17
,, seen in spots and prominences . .	14	.	15
,, not seen ,, ,, . .	1	.	2

The total number of iron lines in the first case is 96. Of those 96 lines only 38, or less than half, are found in the spots and flames. When we go into the lower regions of the solar atmosphere, we leave in fact more than half of the iron lines on one side. Of these 96 lines 15 are found by other observers as well as myself to be common to two or more substances. Now comes the question, what is the behaviour of these common lines with reference to spots and storms? The table shows that among the lines seen in spots and storms fourteen of these basic lines are seen. It must be remembered that our records only give us day by day the results of the 12 most widened lines, and not of all the lines widened. In the next region the number of iron lines is somewhat less—67; 41 of these, or more than half, are picked out by spots and storms; 17 are basic. Of the 17 basic ones 15 are seen in the spots and storms, and only 2 are lines that are not seen.

Next we will turn to another substance, nickel, and there we see very much the same kind of thing at work. In nickel for the region F to *b* we have 20 lines recorded by Thalèn.

Nickel.

	F-*b*
Total number of lines	20
Number in spots and prominences	3
Basic lines	5
,, seen in spots and prominences . . .	3
,, not seen ,, ,, . . .	2

Of these 20, 17 are dropped, abolished; when we come to observe the bright lines and the widened lines of nickel in the spots and storms—the 20 comes down to 3. Among the 20 lines 5 are found to be common to two substances. Of these 3 are seen in the spots or flames, that is to say, every line of nickel seen in a spot or flame is common to two substances.

Passing on to calcium we find the same result.

Calcium.

	F-*b*	*b*-5480
Total number of lines	2	7
Number in spots and prominences	2	5
Basic lines	2	4
„ seen in spots and prominences	2	4
„ not seen „ „	0	0

This is all the work of this nature which we need now consider, but it is not all the work that has been done. In the case of every part of the spectrum, in the case of every substance, the verdict is in the main the same. We have the fact, that two things are observed exactly parallel to each other—first that some lines are common to two substances; next that the lines common to two substances are seen almost exclusively alone, both in the sun-spots and in the prominences. So that in addition to the fact that the hottest regions of the sun seem to simplify the spectra of the substances enormously, we have the further result that the simpler the spectrum becomes, the more complex becomes the origin of the lines; by which I mean that in the ordinary solar spectrum there are a great many lines due to iron, and to nothing else; to calcium, and nothing else; and so on; but the moment we come to the simpler spectrum yielded to us in the spots or the flames, then we have no more right to say that many lines belong to iron than that they belong to titanium, cerium, nickel, and other substances with which those lines have been observed to be coincident with the dispersion employed.

Some of the strongest objections which have been urged against my views have arisen from the fact that the lines which I called "basic" in 1878 have since been found, as I have shown, not to be absolutely coincident, the majority of them being close doubles. I know not whether the views I have put forward above, and the facts I have stated, will be acknowledged to have weight. For my own part, I cannot fail to see here a region where much work is required, and in which, perhaps, some deep secret lies hidden. Much work has been done already by myself, which I have not yet published, though I have referred to it on p. 232. A glance at the facts stated there with regard to iron and titanium will show that doubling the lines is not enough to explain the three- and four-fold coincidences there recorded as the result of four years' work on spectrum purification.

Finally, I remark that the question of dissociation itself is not really so much affected by the basic-line evidence as the other question whether the dissociation has reached a certain term, defined by the presence of common constituents.

The varying intensities of lines, on the other hand, deals with the question of dissociation only, *pur et simple.*

CHAPTER XXVII.

TESTS SUPPLIED BY ARC PHENOMENA.

1. *Line Absorption in the Naked Arc.*

I do not know whether there is on record any observation of line absorption as it is now called, that is of a bright line having a dark absorption line in its centre, earlier than that published in 1872.[1] This referred to an observation made by Dr. Frankland and myself in 1868, which was first demonstrated at the Royal Institution in May 1869. In our researches on sodium we had observed that each of the bright D lines under certain circumstances was traversed by an absorption line sometimes as delicate as a spider's web, sometimes thickening extremely, thereby putting on, in fact, the appearances presented by the F and C lines of hydrogen bright at the sun's limb; a condition of things predicted I believe by Mr. Johnston Stoney before it was actually observed.

Very shortly after the beginning of 1869 I first began the study of the lines seen in the electric arc, an image of the arc being thrown on the slit. The beauty of these line reversals is so great that they have not yet lost their charm for me; even now it is a delight to look at them and watch their changes under the various conditions.

The origin of the phenomena may be thus stated :—

When a mass of metal in the electric arc, in air, is driven into vapour, there are round the incandescent central portion cooler vapours, which, in consequence of their reduced temperature, absorb the light radiated by the poles or by the core of the arc. There are two limits to this absorption. First, the absorbing matter may enter into combination with the gases in the surrounding air, they may be oxidised for instance; or, failing this, they may become more complex and absorb continuously in the blue or red, or finally become liquid or solid by virtue of falling temperature; either of these cases is sufficient to destroy line absorption.

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[1] *Phil. Trans.* 1873, p. 253.

In a journey to Paris I had the satisfaction of discussing these results with my friend Professor Cornu. He afterwards published a list of cases in which he had photographically recorded these phenomena in the vapours of several substances.[1]

When I began to photograph the violet and ultra-violet portions of the spectrum for all the metals easily obtainable, I soon saw that the most striking instances of line reversal were really to be found in this part of the spectrum. Magnesium, aluminium, calcium, strontium, manganese, barium, silver, lead; represents, I think, the order in which these phenomena struck me, now fourteen years ago.

If the method of throwing an image of the arc upon the slit be employed, a method which I suggested and utilised in 1870,[2] for laboratory experiments, there is no difficulty in seeing these reversals.

The longer arc given us by the Siemens' machine enables us, however, not only to note the ordinary phenomena already given at some length in chapter xvi., but to study *where* the reversals take place.

Further, with the Siemens' machine the arc is not only much longer, but when some substances are introduced into it, it is accompanied by a flame sometimes three or four inches long, of great complexity both with regard to colour and concentric envelopes, and we can watch the reversals in the flame as well.

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[1] *Comptes Rendus*, July 21, 1871.

[2] *Phil. Trans.* 1873, p. 254.

The spectrum of this flame I have photographed side by side with that of the arc itself; and when the poles are clean the flame has been shown by eye observation to be in part due to the combination of the carbon vapour of the poles with the nitrogen of the air, thus giving us absolute demonstration of combinations brought about among vapours by reduction of temperature : there is also a new spectrum.

The lines which reverse themselves most readily in the arc are generally those, the absorption of which is most developed in the flame; thus the manganese triplet in the violet is magnificently reversed in the flame, and the blue calcium line is often seen widened, H and K being not only not absorbed, but entirely invisible.

In support of the various statements made in chapter xvi., and as illustrating especially the location of the various absorptions, I may refer to the following photographs[1] in my possession :—

Photographs showing passage from truncation to parallelism.

Spectrum of strontium, showing two reversed lines (wave-lengths 4078·5 and 4215·3) gradually broadening towards one end.

Spectrum of calcium, showing reversal of the blue line, and of H and K. While the blue line presents the appearance of a cone, through the centre of which the absorption line is bounded by parallel sides, the H and K lines are almost normal in their appearance, showing, however, a slight widening at the pole.

Spectrum of manganese, showing the blue calcium line tapering to a point at one extremity and enlarging spindle-shaped towards the other end ; the reversal of this line does not extend through its whole length, but merely through the bulging portion, tapering gradually to a point.

Spectrum of strontium, showing the two lines (4078·5 and 4215·3) which this time present an appearance very similar to the blue calcium line in the last photograph. In the more refrangible line, however, the reversal retaining its tapering form extends through the whole length of the line.

Spectrum of calcium, in which not only the blue line but also the H and K lines present the appearance of truncated cones.

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[1] Exhibited to the Royal Society in 1879.—*Proc. Roy. Soc.* vol. xxviii. p. 431

Spectrum of manganese, showing the absorption of its triplet (at wave-length about 4030) with its radiation.

Spectrum of manganese, in which the triplet is again reversed. Here the triplet, together with its two included bright lines, looks exactly like a group of eight radiation lines, each reversed line giving the appearanc of two bright lines.

Photographs showing non-symmetrical absorption lines in the arc.

Spectrum of silver, showing two lines at about wave-lengths 4054·3 and 4210·0. Both lines are fluffy and reversed ; the less refrangible line is much more strongly expanded on its more refrangible side, and is carried up to a much greater height as a radiation line than its other side. The more refrangible line is more symmetrical, but presents the same phenomena to some extent, only in the opposite direction, its less refrangible side being the most developed.

Spectrum of rubidium, showing line at wave-length 4202. Here the two ends of the line are produced by radiation alone, the central portion showing absorption on its more refrangible side with fluffy shading on its less refrangible side.

Spectra of strontium and calcium, showing the absorption of light due to the poles.

Photograph showing the "trumpeting" of absorption lines—that is, the gradual widening of the absorption line as it recedes from a pole.

Spectra of calcium, in which the reversal is seen to widen as we approach the faint end produced by the cooler external region of the arc, thus showing absorption increasing with reduction of temperature.

Spectrum of lead, showing that the lead line at wave-length 4058 also trumpets.

Spectrum showing the barium line at 4553·4 trumpeting. Here the line, after proceeding to a considerable distance from the hottest region of the arc as a fine reversed line, gradually expands towards its extremity.

Photographs of the spectrum of the flame of the arc.

Flame spectrum of manganese, showing the reversal of the triplet in the flame.

Flame-spectra of calcium, showing the gradual extinction first of K then of H as the flame recedes farthest from the arc.

From these records of the phenomena we learn first, that not all lines of a substance show reversal; secondly, as already stated, that those lines which are most easily reversed, that is to say, which show the absorption most obviously in the arc itself, are those which extend to the greatest distance in the flame; and, thirdly, that we may have indications of absorption in flames at some distance from the arc when the arc lines have become so thin as to render indications of absorption almost impossible, or even after the lines reversed in the arc have disappeared entirely from the flame.

It is perfectly obvious that we have, then, in the electric arc, something not far from an equivalent to a miniature sun; we have a mass of vapours hotter in the centre and cooler outside, and this being so, we have in these reversal phenomena an additional test to apply to the hypothesis. If the elementary substances volatilised in the arc are broken up by the temperature employed, complex molecules should be reformed at the exterior and the simpler molecules should lie nearer the centre, as I have shown in the case of the sun. This, so far as I can see, is a sufficient explanation of all the reversal phenomena that I am familiar with when the naked arc is used; and it includes also an explanation of the phenomena afforded by the flame. It would not be difficult to show that, with one or two exceptions, about which a great deal may be said, the arc picks out for absorption those lines which are not seen in prominences, but which are seen at the lowest temperature at which incandescence takes place.

If the particle subjected to these conditions gives out certain radiations when it is vividly incandescent in the core, we know of no good reason why, if the temperature be sufficient, it should not absorb those radiations when away from the core. The greater the number of exceptions there are to this general statement, the less probable does it become that precisely the same molecule exists in the two conditions.

The simple and sufficient reason of absorption phenomena would seem to be that certain molecules exist in the region of of low temperature more complex than those which exist at a high temperature, and that these molecules are really produced by the association of those finer molecules which exist at the centre of the arc. These finer molecules can exist only in a region close to the arc; the coarser ones are born, and live for a time, in a region further removed.

The lines seen reversed in the flame of a substance incandescent in the naked arc, are, so far as I know in the case of metals of the iron group, not observed in any solar phenomena, although their record is to be found among the Fraunhofer lines. This is also explained by assuming that there as here, they do not exist at all in a highly heated region.

Now, if this molecular dissociation goes on at the temperature of the arc, it is worth while to inquire whether there are other appearances put on when the arc is minutely studied which favour this view.

I now proceed to discuss work of this kind, which, so far as I can see, is entirely in harmony with the above explanation.

2. *Evidences of Separation Exhibited by Bright Lines in the Naked Arc.*

The phenomena presented in the photographs produced by a Siemens' machine are by no means limited to the absorption phenomena which we have just considered. In the case of many substances new spectra altogether made their appearance towards the end of the flame where the lines die out.

This gradual introduction of new spectra, from core to end of flame, led me to imagine that in different regions *of the arc itself* the spectroscopic effects might vary greatly; and to test this I very carefully projected the image of a vertical arc on the slit, focusing that particular light which I was about to observe or photograph.

The greatly increased length of arc obtained by the Siemens' machine has enabled me to observe and photograph a new set of phenomena of great beauty, and I think of the highest theoretical importance. Before I proceed to discuss them it will be well to study still more deeply the conditions actually realised in the arc, and contrast them with those furnished by the sun.

First, if we wish to observe the spectrum of iron, say, in the arc, we have to begin with cold solid iron in the pole, whereas in the hottest strata of the sun cold iron can only get there by accident as it were. In the laboratory the result of the highest temperature, if any special result be produced, will be cloaked, masked, and hidden by all the effects, by all the simplifications, which have been brought about to produce that precise result of the highest temperature. We should get an indication in the arc of the line spectra of all these stages up to the point to which the temperature of the arc is competent to simplify the bodies.

It is next important to point out that if we integrate all the light coming from every part of the arc as we do when we do not throw an image on the slit, we have this masking of possible results in its most intense form. Further, a reminder is necessary, that as in the case of the sun, we are dealing with a globular mass of vapours and not with a section, hence the reasoning in chapter xxii. applies to this case also.

We can make either the upper or lower pole positive, and as the charge will always be in the lower one, if any difference in the spectrum is produced in this way, it can easily be detected if an image is used.

Roughly speaking, the carbons employed give a spectrum containing, besides the flutings of carbon, the lines of calcium and iron, some of them reversed, as I have already shown.

In the photographs of the arc taken under the conditions I have stated, the calcium lines are generally seen at one pole and some of the carbon flutings at the other.

The line which is observed in the line of sight, so to speak, of the pole which does *not* contain the charge is the line which is *not* produced at the highest temperature.

That this is a phenomenon not dependent upon the chemical constitution of the two poles, but rather on some separation, is rendered evident by the fact that on changing the direction of the current, the calcium lines and some of the flutings in the carbon spectra change position.

So much for the spectrum of the poles themselves.

If now we introduce a metal and observe its vapour, we find a perfectly new set of phenomena. We get long and short lines, but the law which they obey is no longer the one in operation when the parts of the arc examined are symmetrical with reference to the positive and negative poles, as when a horizontal arc is employed.

In my former work with a battery of thirty cells, in order to obtain the lengths of the lines, it was necessary, in consequence of the shortness of the arc, to throw an image of a horizontal arc on the vertical slit of the spectroscope. In this manner perfectly symmetrical photographs were obtained, the shortest lines due to the core, and the middle portions of the longest ones, lying in the axis of the photograph.

In the plates obtained under the new conditions, the spectra of those portions of the arc adjacent to the positive and negative poles are widely dissimilar. Some lines stretch across the spectrum with their intensities greatest close to one pole, while other lines, invisible at this pole, are most intense at the other. *In one photograph, for instance, the blue line of calcium is visible alone at one pole, the H and K lines without the blue line, at the other.*

I have shown elsewhere that there is abundant evidence that the H and K lines require a higher temperature to produce them than that necessary for the production of the strong line in the blue.

More than this, there is a progression of lines, so to speak, from pole to pole. They lie *en échelon* along the spectrum.

In the case of other lines, only the central region of the arc is occupied, the line being enormously distended there, either like a spindle or a half-spindle, with the bulging portion in some cases on the more, in others on the less, refrangible side. It is very difficult to understand what process is here at work if we are not in presence of separations brought about by temperature.

The flame phenomena also give us an opportunity of observing the inverse appearance of spectral lines. The following is a case in point:—In one photograph of the flame given by manganese the line at wave-length 4234·5 occurs without the triplet near wave-length 4030, while in other photographs the triplet is present without the line at 4234·5.

The following photographs taken before 1879,[1] may be referred to as showing some details of the phenomena now under discussion.

Photographs showing the spectrum of core and flame of arc compared.

Barium, showing that of two lines (wave-length 4130·5 and 4282·5) of equal intensity in the core, the less refrangible was visible almost alone in the flame.

Strontium, showing two lines at 4078·5 and 4215·3. In the core the more refrangible has a much broader reversal than the other, while in the flame the less refrangible exists alone and reversed.

Manganese, in the flame-spectrum of one of these photographs the triplet at about 4030·0 exists with no other manganese lines; while it is absent from the flame-spectrum of the other photograph, although the manganese line at 4234·8 is present.

Photographs showing reversal of phenomena by reversing the current.

Two spectra of lead, obtained by normal current and reversed current, showing that the lines which, in the upper spectrum were thickened at their lower extremities, were in the lower spectrum thickened at their upper extremities, and that the general appearance of the two sets of lines was reversed.

Two spectra of copper showing the same phenomena.

Photographs showing the lines en échelon in the spectrum.

Spectrum of manganese obtained with the large arc of a Siemens' machine, in which the want of symmetry is so conspicuous that if a straight line be drawn through the centre of the photograph it will cut one set of lines at their centres, another near their upper extremities, while a third set of lines will be cut near their lower extremities.

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[1] *Proc. Roy. Soc.* vol. xxviii. p. 427.

Spectra of titanium, nickel, and manganese in which the lines were not all produced at the two poles, some occupying an intermediate position : the lines thus arranging themselves *en échelon* along the spectrum.

Photographs showing the separation of lines in mixtures.

Spectrum of lithium containing impurity lines of calcium, strontium, iron, and manganese ; the calcium and manganese lines cling to one pole, and the iron and strontium lines to the other.

Different parts of iron spectrum, showing calcium lines starting from one pole, and iron lines from the other.

Photographs showing similar separation of lines in the spectrum of the same substance.

Spectrum of copper showing that this separation can exist not only between two metals, but even between lines of the same metal. In this the blue calcium line is also seen thickened at the upper pole, while the H and K lines were only seen at the lower pole.

Photographs of spectra showing thickening in the centre of the arc in the case of some lines only.

Strontium, containing two lines, one, barium, showing very little thickening, while the other true strontium line exists only at the centre as a broad, fluffy reversed line.

Copper : all the copper lines in this photograph have their central portions expanded, and most of them to a greater extent on the less than on the more refrangible side. Some of the iron lines have also their centres expanded, which is not the case with the calcium lines, those lines in the blue being developed at their extremities.

Magnesium, showing line in the blue-green, with a central expansion on the less refrangible side, giving the line the appearance of a half spindle, *b* being quite normal.

Photographs showing thickening of different lines at different levels.

Spectra of copper and nickel, showing the irregular thickening of different lines at different levels of the arc.

It will be gathered from the observations recorded, that the bright lines seen with the naked arc, concur with the line reversals seen under like conditions, in showing such phenomena as would certainly be produced by separations if separations were effected. This is abundantly demonstrated by the fact that the lines of the same metal behave in exactly the same way as the lines of different metals do when mixtures of metals are rendered incandescent.

3. *Experiments with the Shielded Arc.*

I referred on page 382 to the general conditions which prevent the full development of absorption phenomena when a naked arc is employed.

From what has previously been said, it will be clear that before we can obtain the reversal of any spectral line, which is due to the vibration of a particular molecule, we must interpose between the spectroscope and that molecule a similarly vibrating molecule at lower temperature.

A molecule near the centre of the arc will give bright lines because it is so near the highest temperature ; and in the naked arc, absorption may not be observed because the similar molecules outside it are instantly cooled or oxidised, unless it happen to be a molecule given off in such large quantity with the temperature employed, that these causes of their disappearance are overpowered.

It is obvious that the arc can be shielded from the effects above referred to either by raising the temperature of the surrounding atmosphere or making that atmosphere consist of gases not likely to combine with the vapours of the arc, or both these methods can be combined.

Any experimental means we can employ for studying absorption, should show, when the arc is thus shielded, if my views be correct, that the more we can prolong the existence of these various molecular groupings either by using the arc in an intensely heated chamber or by surrounding it with gases or vapours which prevent chemical change, the more apt will the molecules, judged to be finer by other considerations, be to give indications of absorption, and the better will the intermediate stages be observed. The absorption that will then be observed will gradually include that of lines existing generally at the higher temperatures.

Messrs. Liveing and Dewar have employed many arrangements of this kind, and the results that they have obtained are entirely in harmony with what I should have expected. And, furthermore, many results which they consider extraordinary, are easily and sufficiently explained by the view I have put forward. The variations brought about by the mixing of the vapours with other vapours or gases have been variations due merely to increased absorption when the conditions have favoured such increased absorption. They have in no case produced inversions.

It is the more necessary to refer to these observations, because Messrs. Liveing and Dewar have inferred that they confirm "the theoretical view that alterations of temperature cannot put a stop to any of the fundamental vibrations of a molecule;"[1] and that spectral changes due to varying admixtures may explain the solar phenomena I have already recorded. On this point they write:—

> "There is, however, a further point for consideration which is how far the presence of a mixture of molecules of different elements affects the respective vibrations. This is a condition which obtains in most or all of our observations of the arc in crucibles, as well as in the solar atmosphere, so that it is important to see if any effect can be traced to such a condition of matter. Indeed, in order to arrive at any probable explanation of the variations observed in the spectra of sun-spots and of the chromosphere, we require to study the phenomena produced by such mixtures of vapours as exist in our crucibles, and not merely the spectra produced by the isolated elements either in the spark, arc, or flame."

As far back as 1878 I published the statement that "in encounters of dissimilar molecules the vibrations of each are damped."[2] This damping process will naturally increase the absorption till the absorbing molecule combines.

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[1] *Proc. Roy. Soc.* vol. xxxiii. p. 431

[2] *Studies in Spectrum Analysis*, p. 140.

Again, the observations have been used by Messrs. Liveing and Dewar to invalidate my conclusions with regard to the three iron lines at 4918, 4919·7 and 4923 discussed in chapter xxiv., I hold on the contrary, that their observations form another link in the chain of arguments which can be adduced in favour of my view.

They say :—

> "We have more particularly observed the effect of a current of hydrogen on the iron lines at wave-lengths 4918, 4919·7 and 4923. These lines as seen in the arc in a magnesia crucible usually have about the same relative strengths as are shown in Ångström's map of the solar spectrum ; Thalèn gives their intensities respectively as 2, 1, and 3. They are all developed simultaneously when iron is dropped into the crucible, the first being sometimes reversed, the second frequently reversed for some time, the third much strengthened but not reversed. After a time these effects die out, but if now a very gentle current of hydrogen is led in through one of the carbons perforated for the purpose, the line 4919·7 is again strongly reversed, that at 4918 expanded, while that at 4923 becomes very bright but remains sharply defined. These effects of the hydrogen were observed several times. In all cases the line at wave-length 4923 seemed to maintain about the same relative strength compared with the two other lines, and never showed *any variation*[1] at all corresponding to the prominence it holds in Young's catalogue of chromospheric lines, where it has a frequency of 40, while that at 4918 has only half that frequency, and the strongest line of the three does not figure at all."[2]

With regard to the latter part of this quotation, I may remark that the observations indicate a difference of *kind* between the two more refrangible lines in one case, and the less refrangible one on the other. It is not a question of *degree* as is suggested by the use of the word "variation."

If Messrs. Liveing and Dewar had seen the line at 4923 strongly reversed, the other two lines remaining bright, they certainly might have put this forward as an objection to my view, and I confess it would be very difficult to reply to it.

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[1] The italics are mine.—J. N. L.

[2] *Proc. Roy. Soc.* vol. xxxiii. p. 430.

Messrs. Liveing and Dewar also refer to new absorption lines which are produced when magnesium and sodium vapours, or magnesium and potassium vapours, are mixed.

Such a result is entirely in harmony with my hypothesis, and seems to me to indicate that some of the finer molecules of these substances can combine together at high temperatures and produce new bodies, although it may be impossible to obtain them in the cold; and yet they remark :—[1]

> "The observations on the spectrum of magnesium have a special interest, because from the close analogy of magnesium to zinc and cadmium, it is referred that the molecules of magnesium vapour are chemical atoms of that substance, that is to say, they pass apparently undivided through all the chemical changes to which magnesium may be subjected; and it seems reasonable to suppose, that any subdivision of the chemical atoms could not fail in this case to be attended with a change of chemical qualities, which in the presence of other elements would give rise to new compounds. *No such new compounds have in fact been detected.*[2]

I gather elsewhere[3] that Messrs. Liveing and Dewar have obtained no "independent" evidence of the existence of these compounds. One of the difficulties of the problem of course is, that new substances, even if we acknowledge them to exist at the temperature of the arc, cannot be collected and put into bottles. But this must not be used as a final argument, for if it be, what, as has been well said, would become of the chemical atom and the chemical molecule? Messrs. Liveing and Dewar state moreover, that although they have not obtained the emission spectra in the case of these two substances, supposing them to be compounds, they have in other experiments seen certain lines in their crucibles which require two metals to produce them.[1]

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[1] *Proc. Roy. Soc.* vol. xxxiii. p. 428.

[2] The italics are mine.—J. N. L.

[3] *Proc. Roy. Soc.* vol. xxx. p. 97.

The observations of Messrs. Liveing and Dewar are so important, that I have asked Mr. Taylor, the Demonstrator of Astronomy at the Science Schools, to be good enough to make an abstract of them to print here. Before I give this abstract however, I propose to show what should happen on my view when each method is employed. The reader will then be able to judge whether or no the results in question are in harmony with it.

Chlorine since it destroys the finer molecules by uniting with them, forming chlorides, should prevent reversals ; whereas substances which prevent destruction of the finer molecules enable them to form coarser molecules which reverse.

Nitrous Oxide should produce no marked effects, because although it contains 50 per cent. more oxygen than air, the oxygen in the air is free, while that in the oxide is chemically combined with nitrogen. The chemical attraction has to be overcome before the oxygen can oxidise the finer molecules.

Aluminium at the temperature of the electric arc is known to be one of the most volatile of the metals, and all the photographs taken at Kensington since 1873 confirm this. The addition of aluminium should therefore bring out more reversals of the lines of less volatile and less oxidisable metals by shielding the finer molecules and allowing them to combine.

Magnesium and Potassium are easily volatilised and oxidised, hence they should be very efficient agents in giving reversals, since they are oxidised instead of the finer molecules of the other metals.

Ferrocyanides of the metals, since they contain no oxygen and yield cyanogen when heated, should give more reversals than the metals themselves.

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[1] *Loc. cit.* p. 97.

Coal Gas, which contains oxidisable gases but no oxygen, should act effectively in bringing out reversals, in fact almost as effectively as hydrogen.

Hydrogen, passed into crucibles in just sufficient quantity to burn at the mouth of the tube, should prevent oxidation without diminishing the temperature, and consequently many lines due to finer molecules should be reversed. If the current of hydrogen passed in is too rapid, the cooling effect would to some extent destroy the finer molecules of the metallic vapour and hence they would give no reversals. Hydrogen also, since it prevents the oxidation of the metal and the formation of solid oxidised products which absorb generally, must brighten the continuous spectrum and cause many bright lines in it to become fainter by contrast. Hydrogen should be the most effective of all the agents used for bringing out reversals, since it acts as a shield without producing solid oxidised products.

Abstract of Messrs. Liveing and Dewar's Results.

Calcium as oxide in an arc of thirty Grove cells in a carbon crucible, gives reversals of 4226 and K, but under these conditions H is never reversed. When hydrogen is passed into a Siemens' arc in a lime crucible, the lines 4434, 4425, and 4454 are reversed.[1] Messrs. Liveing and Dewar do not state whether H and K were visible when hydrogen was passed in.

Dark bands frequently appear with ill-defined edges in the positions of the bright green and orange bands.

Aluminium added to the lime reverses 4226, H and K, and the two lines of aluminium between them. Gradually the dark line disappears from H and afterwards from K, while the aluminium lines remain reversed for some time.[2]

A mixture of lime and potassium carbonates (said by Messrs. Liveing and Dewar to produce a strong current of vapour in the tube) in the arc of thirty Grove cells in a carbon crucible, reverses 4425, 4434, and 4454, and the line 4095, a doubtful calcium line.

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[1] *Proc. Roy. Soc.* vol. xxviii. p. 472.

[2] *Ibid.* p. 367.

4454 is most strongly reversed, and remains reversed after the others have disappeared.[1]

It is a significant fact that although K is sometimes reversed without H, H is never reversed without K. In a paper read before the Royal Society in 1879, Mr. Lockyer mentions a photograph which exhibits the blue line of calcium at one pole and the H and K lines without the blue at the other.[2] Again, in further consideration of the reversal of lines observed in the flame of calcium produced in the arc of a Siemens' machine it is noted by Mr. Lockyer that as the vapour recedes from the arc, or in other words as it reaches regions lower in temperature, the H and K lines are gradually extinguished, K disappearing before H.[3]

Messrs. Liveing and Dewar's results in their lime crucibles, are exactly those which Mr. Lockyer had previously obtained, and serve to confirm his views.

Barium.—In an arc of 25 Grove cells in a lime crucible, baryta reverses 5535 (given by Thalèn, as 5534).[4]

Barium chlorate in a carbon crucible in an arc of 30 cells gives 4553, 4933, 5518, and 5535 reversed.[5]

Charred barium tartrate in Siemens' arc in a lime crucible supplied with hydrogen, gives the four lines already mentioned and in addition the line 6496.[6]

Baryta and aluminium in the lime crucible give the same lines reversed as on introducing hydrogen.[7]

Baryta and magnesium in an arc of 25 cells in the lime crucible, reverses the line 5535 along with a line about 4930, which Messrs. Liveing and Dewar say may probably be due to barium.[8]

The nearest barium line in Thalèn's map is 4933, and in Huggins' 4934.

A mixture of barium and potassium carbonates in an arc of 30 cells in carbon crucible, reverses 5535 and 4933.[9]

The apparent contradiction of barium chlorate, is easily explained when we consider the increase of temperature and the enormous volume of carbonic oxide produced when the chlorate is dropped into a carbon crucible. One gram of barium chlorate yields ·4405 litre of carbonic oxide and this acts as a shield for the finer molecules of barium, preventing their oxidation. The chlorine contained in one gram of barium chlorate is ·0734 litre, and this is taken by the impurities (lime and iron) in the carbon crucible.

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[1] *Proc. Roy. Soc.* vol. xxviii. p. 368.

[2] *Ibid.* p. 426. See *ante*, p. 385.

[3] *Ibid.* p. 432.

[4] *Ibid.* p. 357.

[5] *Ibid.* p. 369.

[6] *Ibid.* p. 473.

[7] *Ibid.* p. 473.

[8] *Ibid.* p. 357.

[9] *Ibid.* p. 369.

Lithium carbonate in an arc of 25 cells in the lime crucible reverses the red line only. On adding aluminium, the blue line 4604 (4603 Thalèn) is also reversed, but the green line was only expanded and not reversed.[1]

Using the chloride in the arc and passing in hydrogen, not only the red and blue lines but also the orange and green lines were reversed.[2]

Lithium carbonate in a carbon crucible using the arc of 30 cells, reverses the blue, red, and orange lines on adding aluminium filings and potassium carbonate.[3]

Sodium carbonate in an arc of 25 cells in a carbon crucible reverses the D lines only. Using sodium chloride in a lime crucible with the Siemens' arc and supplying it with hydrogen, the lines 5687, and 5681 were also reversed, the less refrangible one being the more strongly reversed.[4]

Potassium carbonate in a lime crucible with an arc of 25 cells reverses the two extreme red lines and the line 4044, which resolves itself into two lines 4042 and 4045.[5]

On employing a Siemens' arc and passing in hydrogen, in addition to these lines, the lines 5831, 5802, 5782, 6946, 6913, 5353, 5338, 5319, and 5112, are reversed; the lines 5095, and 5081 were not reversed.[6]

Rubidium: charred rubidium tartrate alone in a carbon crucible, gives, with an arc of 30 cells, the two violet lines reversed and the more refrangible of the red lines, 7800. In the Siemens' arc in a lime crucible, the introduction of hydrogen reverses all the above lines, and the less refrangible red one with difficulty.[7]

Strontium tartrate charred and taken alone in a Siemens' arc in a lime crucible, reverses the lines 4812, 4831, and 4873 (4872 Thalèn).

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[1] *Proc. Roy. Soc.* vol. xxviii. p. 357.

[2] *Ibid.* p. 473.

[3] *Ibid.* p. 369.

[4] *Ibid.* p. 473.

[5] *Ibid.* p. 369.

[6] *Ibid.* p. 473.

[7] *Ibid.* p. 369.

On the addition of aluminium, the lines 4962, 4895 (4893, Huggins) and 4868 (4867, Thalèn), were also reversed.[1]

Note.—The naked arc photographed at Kensington in 1875, shows the reversal of 4607·6, 4303·3, and 4215·3.

The line 4895 is not noticed by Thalèn at all, but Huggins gives a line at 4893. It is a question therefore, if this line is not due to impurity, whether 4895 may not be a new line due to a combination of the finer molecules of aluminium and strontium, just as the new lines of magnesium and sodium, magnesium and potassium, and magnesium and hydrogen, as mapped by Messrs. Liveing and Dewar, most probably are.

Lead, introduced as metal into the Siemens' arc in a lime crucible, reverses the line 4058, but lead ferro-cyanide treated in the same way, reverses 4058 and 4032.[2]

Chromium as oxide or bichromate of ammonia in Siemens' arc in the lime crucible gives no reversals. On the introduction of hydrogen, the following lines are easily reversed, viz.:

The green triplet	5207, 5205, 5203.
The indigo triplet	4289, 4274, 4253.
The triplet near N	3578, 3593, 3606.
The double below O about .	3446.
And the triplet	2799·8, 2797, 2794.[3]

Magnesium alone in an arc of 30 cells in a lime crucible does not reverse the *b* lines, but on the addition of aluminium the three *b* lines are reversed in the inverse order of their refrangibility. Magnesia alone does not give the reversal of the *b* lines with an arc of 25 cells, but on adding aluminium the least refrangible member appears reversed.[4]

Manganese sulphate alone in Siemens' arc in a lime crucible, shows the reversal of the violet triplet 4029, 4032, 4033. On the addition of magnesium, the bright blue lines 4823, 4783, and 4753, are reversed, the least refrangible one being most easily reversed.[5]

Note.—The Kensington photographs of the naked arc show the reversal of the violet triplet, and the line 4080 which is not recorded by Messrs. Liveing and Dewar.

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[1] *Proc. Roy. Soc.* vol. xxviii. p. 473.

[2] *Ibid.* vol. xxix. p. 404.

[3] *Ibid.* vol. xxxii. p. 405.

[4] *Ibid.* vol. xxviii. p. 368.

[5] *Ibid.* vol. xxix. p. 404.

Iron.—Iron introduced into the naked arc as metal or chloride gives no reversals, but an iron rod used as positive pole reverses the line 4045. By putting iron wire through the perforated carbon pole into the arc of a Siemens' machine in a lime crucible, the lines reversed are 4045, 4063, 4071, 4325, 4383, 4404, 4920, and 4957.[1]

On adding magnesium to the lime crucible containing iron, the iron lines about L and M, four strong lines below N, the line O, all the strong lines from S_2 to U inclusive, and the two groups still more refrangible, are reversed.

There is an increase of continuous spectrum on adding magnesium, just as there is on adding hydrogen.[2]

Ferro-cyanide of potassium, $K_4FeC_6N_6$, in Siemens' arc in a lime crucible gives exactly the same reversals as were produced by the addition of magnesium. On passing hydrogen into the crucible, no less than 140 lines were reversed.[3]

Note.—Photographs taken at Kensington in 1877 clearly show the reversal of 4045, 4063, and 4071 in the naked arc.

Messrs. Liveing and Dewar have frequently expanded lines without reversing them by using mixtures of metals in their crucibles. Nickel alone in a magnesia crucible with the Siemens' arc does not give its lines very strongly developed, but on introducing several other metals, such as iron, chromium, &c., the nickel lines come out with great brilliance and considerably expanded, remaining so for some time.[4]

An alloy of manganese, iron and titanium, had the effect of making the nickel line broad and diffuse.[5]

Here the nickel molecules are to some extent protected, sufficient in fact to cause the lines to expand, which is always a preliminary to reversal, but since no very good shielding atmosphere like coal gas or hydrogen was used, no reversals were obtained.

The above results whenever three experiments have been made under different conditions with the same metal are collected in the following table :—

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[1] *Proc. Roy. Soc.* vol. xxix. p. 405.

[2] *Ibid.* vol. xxxii. p. 403.

[3] *Ibid.* p. 403.

[4] *Ibid.* vol. xxxiii. p. 431.

[5] *Ibid.* p. 434.

Metal.	*Alone.*	With Magnesium.	With Potassium carbonate.	With Aluminium.	With Hydrogen or coal gas.
Calcium.	3934(K)	—		3934(K)	?
	—	—	?	3968(H)	
	4226	—		4226	4226
	—	—	4425	4425	4425
	—	—	4434	4434	4434
	--	—	4454	4454	4454
Barium.	—	—	—	4553	4553
	—	—	4933	4933	4933
	—	—	—	5518	5518
	5535	5535	5535	5535	5535
		—	—	6496	6496
Lithium.	6705	--	—	6705	6705
	—	—	—	6102	6102
	—	—	—	—	4972
	—	—	—	4604	4604
Magnesium.	No *b* lines.	—	—	One *b* the least refrangible.	The three *b* lines reversed.
Iron.	No reversals.	Many lines in ultra violet reversed.	—	—	140 lines reversed.

The results obtained when only two experiments with the same metal under different conditions have been made are as follows :

	Lines seen with metal alone in crucible.	Lines seen in arc shielded by hydrogen.
Sodium	2	4
Potassium	4	11
Rubidium	3	4
Strontium	3	6
Chromium	0	13
		Shielded by ferro-cyanide.
Lead	1	2
		Shielded by magnesium.
Manganese	3	6

The tables at the end of the above abstract, show how very simple is the result obtained in Messrs. Liveing and Dewar's very interesting experiments. We get an increase of absorption when the conditions are favourable to the lives of the absorbing molecules, *voilà tout*.

The results throw light in addition on some of the questions which the experimenters had in their minds when the experiments were undertaken, including the theoretical view that alterations of temperature cannot put a stop to any of the fundamental vibrations of a molecule. It is clear that the vibrations which produce absorption are put a stop to in the case of iron for instance, when no absorptions are seen, although they exist when the absorption of 140 lines is seen. One inference at all events is that the molecules themselves are "put a stop to" under the conditions of the naked arc. It must be said at the same time however that the "study of phenomena produced by such mixtures of vapour as exist in our crucibles" from which they hoped so much touching the "probable explanation of the variations observed in the spectra of sun-spots and the chromosphere" does not help us in that direction in the least.

With regard to the sun-spot spectra I may remark, that I have discussed the absorption lines of iron in hydrogen observed by Messrs. Liveing and Dewar in connection with the Kensington sun-spot observations. In the region included from F to *b* according to Thalèn's new list there are 155 lines of iron. Of these fifty-nine have been seen among the most widened lines of iron at the sun-spot minimum. Over the same region Messrs. Liveing and Dewar have seen reversed ten, all of which have been seen among the most widened lines at different times. If temperature then has not put a stop to the vibrations of the remaining 145, it would be interesting to imagine what cause is more probable than the one I have suggested, and it evidently would be a very important experiment to see what the absorption in a larger crucible would give us.

The pith and marrow of the work referred to in this chapter may be stated as follows :—

(1) At the highest temperatures we can get at here, or are acquainted with in the celestial bodies, the most orthodox chemists agree we are dealing with the chemical "atom."

(2) Most chemists will accept Clerk-Maxwell's definition of an atom that it is "a thing which cannot be cut in two" to which I will add that it cannot be doing two opposite things at the same time.

(3) The line spectrum is held to be the radiation from the "atom" of a chemical substance.

(4) We have found the spectra of the same substance in spots and prominences to be complementary to each other. That is to say, in both cases certain vibrations of the same "atom" have been stopped.

(5) We have found the thing which cannot be cut in two indicating rest, and a motion of thirty miles a second, at the same time.

(6) We have found the "atom" of calcium, a thing which cannot be doing two opposite things at the same time, in sun-spots giving as H and K bright in photographs while the blue line is absorbing.

(7) We now find that the "atoms" surrounding the core of the arc absorb only certain radiations.

(8) We now find that similar "atoms" vibrate with certain wave-lengths at one pole of the arc, and with different wave-lengths at the other.

It must not be forgotten that the vibration of an "atom" according to the received views is a very serious affair. The vibration of the atom of cerium for instance according to that view is more complex than all the vibrations of all the substances in the solar atmosphere put together; for the number of lines in the spectrum of cerium (the specimen was supplied to me by my friend Dr. Holtzmann as *puerissimo*, having been prepared in Bunsen's laboratory) between G and K is greater than the number of Fraunhofer lines in that region.

On this point I cannot refrain from quoting the following remarks by Mr. Liveing :—

> "The spectrum of iron . . . . presents thousands of lines distributed irregularly through the whole length not only of the visible but of the ultra-violet regions. Make what allowance you please for unknown harmonic relations, and for lines which are not reversible and may not be directly due to vibrations of molecules, we still have a number of vibrations so immense that we can hardly conceive any single molecule to be capable of all of them, and are almost driven to ascribe them to a mixture of differing molecules, though we have as yet no independent evidence of this."

On this extract I need only remark that if the elementary bodies are incapable of separation into their constituents by ordinary chemical processes, and yet are decomposable, the spectroscopic phenomena treated of in this book are precisely those we should expect supposing that high temperatures do really dissociate. This evidence is not to be neglected because the chemist is slack in producing "independent" evidence. Mr. Crookes' recent work suggests that the time is not far distant when such "independent" evidence will be before the court.

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[1] *British Association Report*, 1882, p. 483.

CHAPTER XXVIII.

APPLICATION OF THE HYPOTHESIS TO THE GENERAL PHENOMENA OF THE SUN.

I TRUST my readers are not yet tired of tests. My excuse for my insistence upon them must be that I hold it to be the duty of a student of science who suggests a new view, to spend as much of the rest of his life as is necessary to determine whether it is true or false.

We have seen in the last few chapters how the new view fits the facts, so far as I have been able to follow them, in the spectroscopic examination of spots and prominences and of the sun's atmosphere during an eclipse, as well as in laboratory work.

I propose now to go further afield and see whether the hypothesis breaks down or gives us light when we apply it to the explanation of ordinary solar phenomena, when, in short, we pass from solar chemistry to solar physics.

We start with the view that there is a true atmosphere arranged in layers, the chemical components of which depend upon temperature only. We get association as particles move up from A to L, dissociation as particles move down from L to A. Each shell will be hotter than the superincumbent one, and hence if for a moment we neglect the effect of pressure, the sun as bounded by the photosphere will be a mass of gas at a far higher temperature than anything outside it.

This brings us face to face with the density of this mass of gas. Look at the first test.

1. *By the hypothesis the density of the sun must be low.*

We are accustomed, in dealing with the earth and comparing it with other heavenly bodies, to refer to their density. We say, for instance, that the density of the earth is five and a half times greater than the density of water; that is to say, that the earth put in one scale-pan would weigh down five and a half earths of the same size, if they were made of water, put in the other. And we say, further, that the density of the earth is about the same as the density of Venus and of Mars; but the density of the other planets is very much less. We know on the earth that water is less dense, for instance, than mercury. We know that spirit is less dense than water. All these represent

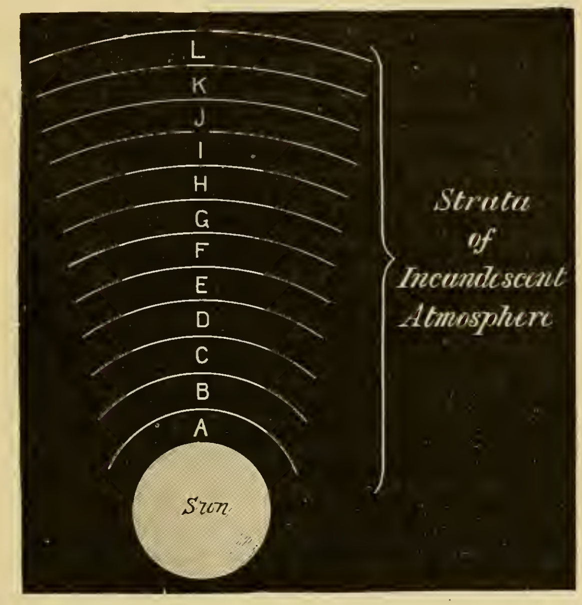

FIG. 120.—Hypothetical section of the solar atmosphere.

different orders of density. The same thing happens with regard to gases. We know that hydrogen is less dense than oxygen and nitrogen, and so on.

Now, what is the density of the sun? Is the sun denser than the earth? No; according to the books it is just about a quarter as dense as the earth, so that it is a little denser than water. In fact, if we take water as our unit of density, the density of the sun is 1·444. If we take the density of the earth as one, then the value for the sun is about 0·25.

These are the values given in the books, but they have been determined by taking the volume of the sun as given by the diameter of the photosphere—860,000 miles. But if we concede 100,000 miles for the height of the layers of atmosphere above the photosphere, those layers must not be left out of consideration. If we include these layers, though we do not alter the mass, we alter the volume. If we put the same mass into a larger volume, we naturally reduce the density. Now, if we take the atmosphere of the sun as extending to 100,000 miles above the photosphere, that will give us a radius of 530,000 miles, instead of 430,000 miles, and we shall, as nearly as may be, double the sun's volume. Therefore we shall have halved the density. Instead of being a quarter as dense as the earth, it will only be one-eighth as dense; and, instead of being just denser than water, it will be a little over half the density of water. For my own part, I think that this 100,000 miles is not sufficient. I think that it is the minimum, and that most students of solar physics would agree that a height for this purpose of 500,000 miles above the photosphere would be probably nearer the mark. That will give us exactly ten times the volume of the sun bounded by the photosphere, so that the mean density will be reduced to the tenth; we shall get a mean density then of about one-eighth that of water. This, of course, is the average density of the whole volume in which the mass is supposed to be diffused—the mass which is a fact which we cannot get out of, and which has a definite relation to the mass of our own earth.

Now if these arguments are of any value we must concede that the mean density of the sun is very low indeed, much lower than that of any planet or satellite with which we are acquainted; so that we are perfectly justified in saying that it is an enormous globe of gas, by which I do not mean that it is absolutely and completely gaseous to the core. The gases of the centre—gases under very great pressure—may put on the appearance, if they do not put on all the physical properties, of liquids.

It will be seen that this revised value of the sun's mean density is quite in accordance with the hypothesis. There is another very important consideration which depends naturally upon this revised value of the mean density, and which to clear the ground we must refer to it in this place.

The photosphere, as already stated, is about 400,000 miles—in round numbers—from the sun's centre, and has an outer atmosphere 500,000 miles high above it. If we take the lower average density of the sun we note that the photosphere, assuming it to be a shell, exists in a region of low pressure, and we see in a moment that, unless we suppose the photosphere, or something immediately inside the photosphere, to be solid, there is no reason for supposing any *sudden* increase of pressure at the photosphere itself. In fact, there are a great many reasons for regarding this as improbable, not to say impossible, and the lower the density the less likely is it.

Hence, any changes at the photospheric level must be due to something else than pressure, in all probability to temperature; still of course the pressure below will be higher than that above, and the line of least resistance will be upwards.

The ground then having been cleared, let us consider the shells in a condition of perfect equilibrium; there will be no currents either up or down.

Now assume a disturbance. Where is it most likely to arise? It is most difficult to imagine how a disturbance can arise in the centre of such a globe of gas as we are considering in a state of perfect equilibrium, but on the other hand it is quite easy to see that such a disturbance is very likely to arise among the exterior layers, either from collisions among the solid particles which must exist there mingled with the permanent gases in consequence of the lower temperature of those layers, or from some action from without, such as the planetary action suggested by De la Rue, Stewart and Loewy. Hence:—

2. *By the hypothesis, in any given solar area of disturbance of the convection equilibrium a down-rush of cooler materials produced by gravitation must begin the disturbance.*

Now what are the facts? There is much evidence to show that a spot is the first disturbance of the photosphere in the region where it is formed. I mean *the faculæ and the movements in the general surface follow, and do not precede the formation of a spot.* On this point I first quote Dr. Peters,[1] one of our highest authorities:—

> "The spots arise from insensible points, so that the exact moment of their origin cannot be stated; but they grow very rapidly in the beginning, and almost always in less than a day they arrive at their maximum of size. Then they are stationary, I would say, in the vigorous epoch of their life, with a well-defined penumbra of regular and rather simple shape. So they sustain themselves for ten, twenty, and some even for fifty days."

I next quote the Rev. S. J. Perry, one of the most constant of modern solar observers:—

> "And, to begin with spot-formation, we find almost invariably that large solar spots start life as little dots, frequently in groups, and then grow at once with enormous rapidity. A spot will often

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[1] *Proceedings* of the American Association for the Advancement of Science, vol. ix.

attain its full size in five or six days, although exceptionally large ones occasionally occupy a longer time in their first development. If no remarkable increase is noticed in a spot within two or three days from its birth, it will in all probability never attain to any considerable size. The solar surface has repeatedly been examined with the greatest care, in regions where considerable spots have broken out on the following day, without detecting any marked disturbance or other sign announcing a probable outburst. No satisfactory exceptions to this have as yet been noticed."

Professor Spörer, on the other hand,[1] is inclined to think that spots form only on the more luminous parts of the photosphere, that is in regions of higher temperature; and indeed I gather that in his view an up-rush begins the disturbance. But, even if his observation be correct—and it differs, as will be seen, fundamentally from those of Perry (see above quotation), the greater illumination of the photosphere may be explained by supposing that the great down-rush which produces a spot may be preceded by slighter falls which disturb the photosphere without giving rise to large masses of absorbing vapour or the appearance of faculæ.

With regard to the absorbing vapour itself it must not be forgotten that it must be at a very high temperature.

Professor Hastings of Baltimore[2] thinks it is something like smoke, but this is negatived by the spectrum observation made and recorded years ago. On this point Professor Young has lately written as follows:—

"With a high dispersion the darkest part of the spot-spectrum is found to be not continuous, but made up of fine lines overlapping or almost touching each other, with here and there a clear space left, like a fine bright line. It means, I think, that the absorbing vapours which darken the interior of the spot are wholly gaseous, and tends to disprove the idea that they are mostly of the nature of smoke or steam. We mention also, in passing, another thing which has been shown by our large instrument at Princeton—that the apparently bulbous, finger-tip-like terminations of the penumbral filaments are often, under the best circumstances of vision, resolved into fine, bright, sharp pointed hooks which look like the tips of curling flames."[1]

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[1] *Nature*, Nov. 18, 1886, page 72.

[2] *American Journal of Science*, Jan. 1881.

Professor Hastings seems to have found his smoke view of sun-spots strengthened by the appearance of the edge of the penumbra. To the eye the outer edge, where the half tone is in contrast with the photosphere, seems darker than the one which is in contrast with the black nucleus. This is a subjective appearance merely; as shown in photographs, the inner edge is *not* brightened.

3. *By the hypothesis such falls of cooled material must take place in different degrees and in greater or less quantity on all parts of the solar surface.*

From this it follows that there should be observed on the sun very definite phenomena of the same kind but very different in degree; and that if some of the phenomena are limited to certain regions, as we shall afterwards find them to be, others are universally distributed over the sun's surface. As a matter of fact such definite phenomena differing only in degree are well known. I refer to each in turn, beginning with the most general.

(1) *Pores.*—When we look at the sun what one sees is first a bright disc, which is slightly dimmed at the edge; on examining the disc carefully, what we further see is a strange mottling of the whole surface. The mottling is very often very delicate; but everywhere, in all parts of the sun, near the poles, near the solar equator, and universally, we get this strange mottling. Fine dots called granulations or pores are everywhere visible, and each pore

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[1] Young, *Nature*, Nov. 17, 1886.

when examined by the spectroscope is found to be a true spot. These fine mottlings sometimes take certain directions, in consequence of the existence of powerful currents. Here and there we get cyclonic swirls, and here and there there is an appearance of smudginess, apparently produced by tremendous overhead currents, so to speak, that is, currents between us and that part of the sun on which they appear.

(2) *Veiled spots.*—In addition to these pores M. Trouvelot discovered about ten years ago a kind of spot which he calls *veiled spots;* in those regions where they occur his opinion is that the photosphere is driven down to a certain extent, but not driven down sufficiently to give us the dark appearance which we get in the spots of the ordinary kind.

Some spots again take the appearance of enlarged pores, that is they have no penumbra, and others again appear as intensified veiled spots, that is to say they have no umbra, but the penumbral shade is very well developed.

Independently, then, of the phenomena of the large obvious so-called "spots," ample evidence has been secured by minute observation to show that the whole photosphere is traversed by down-rushes, some of them possibly of almost infinitesimal dimensions, and others varying in importance till the most tremendous manifestations of the action are developed.

When we come to examine these spots carefully, we find that there are apparently in the main—(I wish to speak as guardedly as I can)—two different kinds.

(3) *Quiet spots.*—Some spots seem to be pretty regular, and to undergo no very violent commotion. I mean that the penumbra and the umbra are not so tremendously contorted and mixed up as sometimes happens; and again, the ridge of facula round the spot is not so honeycombed by convection currents and lateral currents and their results.

(4) *Disturbed spots.*—On the other hand, as representing the other class where we get violent action, there seems to be no

limit to the enormous energies indicated, and the areas over which these energies hold their sway. One spot, or at least a spot system, was observed in 1858, of 140,000 miles or eighteen earth-diameters in length. Telescopic examination of each minute part of these enormous disturbances indicates that the most violent changes are going on—changes which the eye can detect, after a few minutes' interval, in different parts of the spot.

These changes present us with phenomena of reaction, as if the material carried in the first instance below the upper level of the photosphere had produced a disturbance in the lower regions, followed by the increase in the quantity and brilliancy of the faculous matter and its separation into small masses, while at times the umbra of the spot becomes distinctly coloured.

I again quote Dr. Peters :—

> "The notches in the margin, which, with a high magnifying power, always appear somewhat serrate, grow deeper, to such a degree that the penumbra in some parts becomes interrupted by straight and narrow luminous tracts—already the period of decadence is approaching. This begins with the following highly interesting phenomenon. Two of the notches from opposite sides step forward into the area, over-roofing even a part of the nucleus; and suddenly from their prominent points flashes go out, meeting each other on their way, hanging together for a moment, then breaking off and receding to their points of starting. Soon this electric play begins anew and continues for a few minutes, ending finally with the connection of the two notches, thus establishing a bridge and dividing the spot into two parts. Only once I had the fortune to witness the occurrence between three advanced points. Here from the point A a flash proceeded towards B, which sent forth a ray to meet the former when this had arrived very near. Soon this seemed saturated, and was suddenly repelled; however, it did not retire, but bent with a sudden swing toward C; then again, in the same manner, as by repulsion and attraction, it returned to B;

and, after having thus oscillated for several times, A adhered at last permanently to B. . . . The flashes proceeded with great speed, but not so that the eye might not follow them distinctly. By an estimation of time and known dimension of space traversed, at least an under limit of the velocity may be found; thus, I compute this velocity to be not less than 200,000,000 metres [or about 120,000 miles] in a second.

"The process described is accomplished in the higher photosphere, and seems not to affect at all the lower or dark atmosphere. With it a second, or rather a third, period in the spot's life has begun, that of dissolution, which lasts sometimes for ten or twenty days, during which time the components are again subdivided, while the other parts of the luminous margin, too, are pressing, diminishing, and finally overcasting the whole, thus ending the ephemeral existence of the spot.

"Rather a good chance is required for observing the remarkable phenomenon which introduces the covering process, since it is achieved in a few minutes, and it demands, moreover, a perfectly calm atmosphere, in order not to be confounded with a kind of scintillation which is perceived very often in the spots, especially with fatigued eyes. The observer ought to watch for it under otherwise favourable circumstances when a large and ten or twenty-days'-old spot begins to show strong indentations on the margin."

The scintillation referred to by Dr. Peters is perhaps associated with a phenomenon which has been described by M. Trouvelot,[1] who has observed the faculous masses to subdivide into small flakes which vibrate rapidly, producing the effect of a snow-storm above the umbra, when these dissolve into blue or violet vapours.

It happens sometimes also near groups of spots which are endowed with great activity that perturbations are observed which are so violent that the adjacent photosphere is shaken to its foundations, cracks, and, on opening, forms sinuous crevasses which extend to considerable distances, sometimes connecting the most distant spots with each other.

[1] *Bulletin Astronomique*, vol. ii.

The last act in the history of a spot is its invasion by the faculæ. These remain long after the spot has entirely closed up, and in this connection it is important to remark that new spots very often break out in the old place. These of course, unlike the first spot in that position, will appear to be preceded by faculæ.

We have then the following generic grading of the effects produced by the descending material, in the order of intensity:—

(1) Pores.

(2) Veiled spots.

(3) Small quiet spots with penumbræ and umbræ.

(4) Large areas containing several spots, giving evidence of violent disturbance.

4. *By the hypothesis this falling material is dissociated in its descent before or when it reaches the photosphere; the particles which descend sparsely and gently will be vaporized gently, and those which descend violently and in great masses will be exploded violently.*

We have before seen that the down-rush is of course produced by gravitation. The velocity generated in the fall gives us kinetic energy in the moving gas, and as this velocity (which, neglecting friction, must reach many miles a second) is checked by the surrounding gases, the kinetic energy becomes heat. This heat produces sudden expansions, and the central initial down-rush is now surrounded by up-rushes.

These effects will differ in degree, depending upon the amount of descending material and the height of fall, and we shall get different appearances due to these various degrees.

On this point the history is quite complete.

(1) *Domes.*—All over the surface are seen domes of faculæ, either separate, or combined in masses of greater or less importance; and when the definition is quite perfect it is noted that

these domes are hotter than the rest of the surface, since the bright lines of hydrogen are seen to surmount them. It is quite possible that the general billowy outline of the chromosphere when most quiescent is due to this cause.

(2) *Metallic strata.*—The next increase in the action is evidenced by a more complicated outline of the chromosphere, accompanied by a more complicated structure chemically.

Occasionally the level of this sea over a very large region is gradually, peacefully, and quietly raised, and when that happens

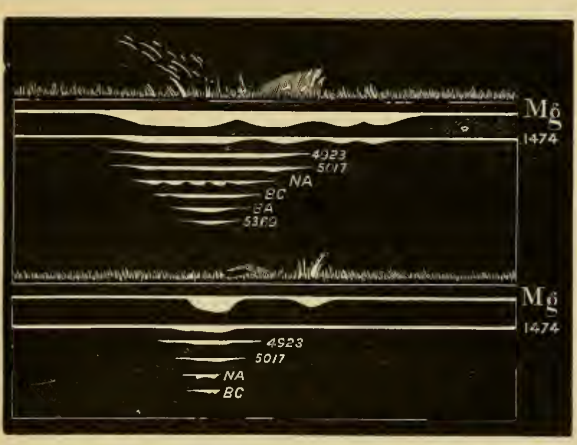

FIG. 121.—Wellings-up of metallic strata. (Tacchini.)

observations show us that this apparent welling-up is due to the intrusion of other vapours. There seems to be a gradual evaporation from out the photosphere, or a gradual heating of slowly-falling material over large regions, pushing up the upper level of the sea of hydrogen, so that the portion of the atmosphere near the photosphere gets richer and richer; we get, in fact, layers of different substances above it.

Above is a drawing showing two wellings-up of metallic strata in the chromosphere to which I have referred. The distance from the horizontal line shows the depth of the strata indicated by the lengths of the various lines. The stratum which reaches highest up has a spectrum containing a certain line of magnesium. The next, which is shallower, consists of a substance about which we know nothing, except that its line is called "1474." Then we get other shallower strata. These, again, are of unknown origin. The lower we go, the deeper does the mystery become.

(3) *Quiet prominences.*—The next degree of disturbance shows itself in a different way, but very frequently in a region where the first beginnings of a disturbance of the temperature equilibrium, produced by falling material, had been evidenced in the manner just described; we get the formation of what is called a *quiet* prominence.

As a rule they need not be very high. By very high I mean 40,000 miles. And these quiet prominences may also last for a very long time. Many of them resemble trees. I was fortunate enough to be one of the early observers of these exquisite forms, which one never gets tired of looking at, and the first time I saw one I wrote down in my note-book that the chromosphere and prominences in that place reminded me of an English hedge-row with luxuriant elms. The lower part of the chromosphere, of course, represented the hedge, and the prominences the elms. The simile of a hedge with trees in it was not at all a bad one, but some years afterwards I found a very much better one, which is perhaps nearer the truth of Nature. It was my duty in the year 1878 to go to America to look at an eclipse. I crossed the Atlantic in the high summer, and we naturally had to pass through a considerable amount of fog. We were three days in a dense fog, and one of the delightful things about that fog was that one day we were steaming through an opening, and saw its edge, which was apparently upright and solid, about a mile off, and we coasted along it.

I found that the fog was fed by what I at once called fog-spouts. Everybody knows what water-spouts are, and we have all seen drawings of them, and the drawings of waterspouts that I have seen represent the reality very well. If we imagine a bank of fog about fifty or sixty feet high filled with little fog-spouts, we get exactly what I then saw, and what one often sees in quiet prominences on the sun. It may be that what I and others have likened to the trunks of trees may be really somewhat akin to these fog-spouts, with the enormous difference, however, that we are dealing with water and aqueous vapour in one case, and with the photosphere of the sun and incandescent hydrogen gas in the other.

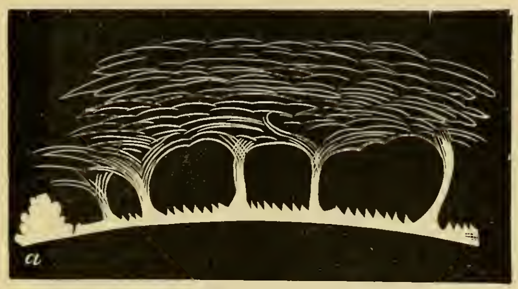

FIG. 122.—Tree-like prominences.

These quiet prominences are built up entirely of hydrogen.[1] When I say "quiet" it must be understood that the word is a relative one. I have seen a quiet prominence as big as a dozen earths born and die in an hour. That is not at all an uncommon thing. And there are several facts which indicate that when such a prominence disappears, it does not mean that the stuff disappears; it means that it changes its state, that is to say, it chiefly changes its temperature.[2]

[1] That is a substance which gives us some of the lines observed when hydrogen is examined. I do not mean that the hydrogen is such as we know of here.

[2] M. Trouvelot states that he observed at Meudon at 9h. 25m. on Aug. 16, 1885, a brilliant prominence 4′, or 108,000 miles, in height, which, two hours later, had grown to 9′ 27″. That is to say, it then reached to at least (for it was probably to some extent foreshortened) a quarter of a million miles from the

When we watch the growth of such a prominence it expands from below, close to the photosphere. First the prominence is of small height, then it gets higher and generally broader, and after a certain time we may see a kind of cloud formed at the top of it.

It happens very rarely indeed that any very rapid vertical motion is indicated in such prominences as these.

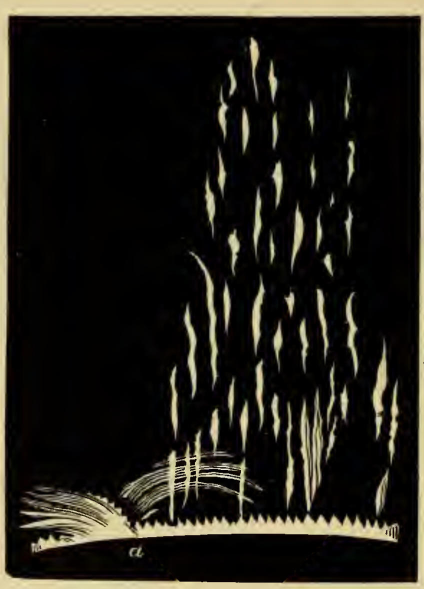

FIG. 123.—Metallic prominence, Young. Rate of ascent 400,000 miles an hour.

(4) *Metallic prominences.*—The most intense degree of action is observed in what are termed metallic prominences, for the reason that they are known to include other substances besides hydrogen.

These are the most magnificent manifestation of energy which

photosphere. But so rapid was the cooling which attended this rapid expansion, that in *two minutes* after it was last measured it had completely vanished.—*L'Astronomie*, vol. iv. p. 441.

it is given to man to witness, and very impressive indeed are the phenomena recorded. These metallic prominences are at times observed shooting up almost instantaneously—the exact rate of motion I will state by and by—to enormous heights; and not only are they seen to shoot up into the atmosphere with very great velocity and with every indication of the most violent disturbance, but their ascent is accompanied by most violent lateral motions. We have means, both by actual observation in the case of the up-rise of the prominences into the solar air and in the change of the wave-lengths of the lines in the case of any lateral motion, of determining how fast these violent prominences rise and are driven by solar winds.

Such prominences have been seen to mount upwards at the rate of 250 miles a second, that is nearly 1,000,000 miles an hour; so that, if these gases continued their flight they would reach the top of the solar atmosphere, if the solar atmosphere were 1,000,000 miles deep from the top down to the photosphere, in about an hour's time. There are indications that these prominences, instead of rising vertically, as we may imagine them to do, are at times shot out sideways—almost tangentially. In that case, of course, the spectroscope enables us to determine the velocity. 100 miles a second, either towards or from the eye, is by no means an uncommon velocity, and there are also indications that, in the neighbourhood of the photosphere, where these enormous prominences take their rise, vividly incandescent hydrogen at a considerable pressure is present.

The height of some of these prominences is very great. Professor Young records one seen in 1878 as being nearly 400,000 miles high, that is 13½ minutes of arc, the solar radius being 16 minutes.

We have then, to sum up the effects of the ascents in the order of intensity:—

1. Domes.
2. Metallic strata.
3. Quiet prominences.
4. Metallic prominences.

5. *By the hypothesis, the effects, in various degree, produced by the falls of associated material must be related to the effects, in like degree, produced by the ascents of dissociated material.*

The facts with regard to both these classes of phenomena have already been stated; we will here give them again in parallel columns:—

Effects of Falls.	Effects of Ascents.
1. Pores.	1. Domes.
2. Veiled spots.	2. Metallic strata and small prominences.
3. Quiet spots.	3. Quiet prominences.
4. Disturbed spots.	4. Metallic prominences.

Now every observer of solar phenomena is perfectly conversant with the fact that the pores and the domes are most closely associated in every solar region, and that the domes are always seen in their greatest intensity as lines or masses of faculæ near spots *or where spots have been.*

Observers armed even with small spectroscopes have found that it is almost impossible to see a large spot at the edge of the sun, which is the place for observing it best, without finding the down-rush towards the photosphere of cool, absorbing spot-producing material, accompanied by a metallic prominence which indeed may consist of both descending and ascending materials.

There is one word which expresses, as well as anything I can think of, the impression which is made on one by the combined phenomena. There is a *splash.*

Imagine an enormous cauldron of liquid iron, as hot as you like. Play some water into it from a hose; there will be a splash. The water, of course, would be very violently heated; we probably might get some explosions, and as the result of these explosions some liquid iron might be carried with the liquid water which has entered into the liquid iron here and there.

There is the strictest relationship of intensity between the action going on in a spot and its associated prominence, so much so that when there is any very violent change indicated in a spot on the disc the associated metallic prominence is at times seen bright upon the sun. In this case there is little doubt that the prominence is an up-rush pure and simple.

But we can go further than this.

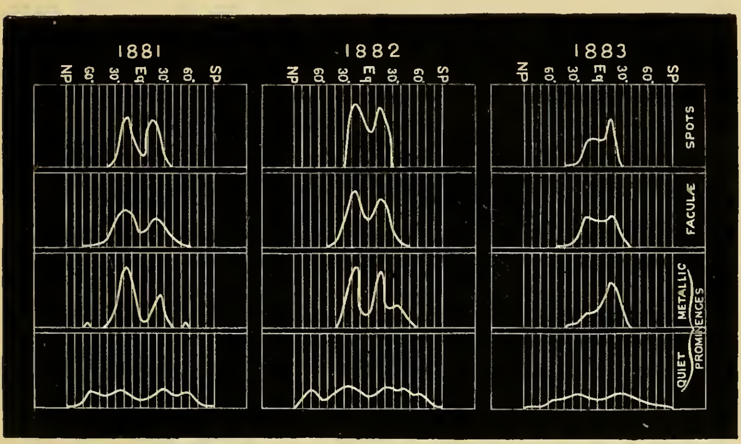

Fig. 124.—Diagram summarising the results of the Italian observations for the years 1881-83.

6. *By the hypothesis, if any phenomena caused by falls occupy any special zone on the sun, the associated ascending phenomena should be limited to the same zone at the same time.*

Now the most general phenomena are :—

Pores	replied to by . .	Domes.
Veiled spots . .	,, ,,	Metallic strata.
Quiet spots . .	,, ,,	Quiet prominences.

These first three are seen in every part of the sun's surface. But when we come to :—

Disturbed spots . replied to by . . Metallic prominences, the zone in which such spots (see page 409 *et seq.*) appear is limited and is liable to change: these conditions therefore should hold with the metallic prominences.

Again, what are the facts, always remembering that the spot appears first in point of time?

The Italian observers have not only very carefully observed the prominences from day to day, but they have observed spots and the other phenomena which require continuous investigation. The foregoing diagram puts together in a very convenient form much information which we want in this place. The information extends over three years, so that we have not merely to depend on the result of one year's observation. The curves have a very simple explanation. In the middle of each yearly series E stands for the equator, and right and left of that we have vertical lines giving every 10° of latitude from the equator to the poles south or north for each year. The height of the curves from the base-line represents the number either of spots, faculæ, metallic or quiet prominences, seen each year. The spots in the year 1881 had their maximum in latitude 20° N., and 12° S. There were no spots either north or south of latitude 40°, and there were very few spots indeed near the equator of the sun. In 1882 the conditions are a little changed. There are some spots near the equator, and the maximum of spots now is 18° N., and there are more spots this year than there were last, because the curve is higher. Going on to 1883 the maximum of spots has changed from the north of the equator to the south, and in latitude 15° S. we have a reduced maximum, whereas in the northern hemisphere we get very nearly the same quantity in latitude 10° and 20°.

The other curves may now be compared with these, and the point of enormous importance is this, that the maximum of the faculæ and of the metallic prominences agree absolutely in position with that of the spots. When one phenomenon changes its position on the sun the other does so also.

When and where the spots are at the maximum, the faculæ and the metallic prominences are at the maximum. If the maximum changes from north to south, as it does, in the spots; it changes from north to south in the metallic prominences; and from north to south in the faculæ. So that were we dependent on these diagrams alone, representing three years' work, we should be driven to the conclusion that there is absolutely the most intimate and important connection between spots, the metallic prominences, and the faculæ; and not only that; we reach finally the fact of the wonderful localisation of these phenomena upon the sun. The spots are never seen north or south of 40°.[1] They are invariably seen in smaller quantity at the equator. Similarly while the domes are small all over the sun, the brighter collections of them, the faculæ, do not go very much further than 40° north or south, and their minimum is also at the equator. The metallic prominences also never go very much beyond the spot region, and they also have a minimum at the equator.

But when we pass to the prominences of the quiet sort that is not so. They, like the domes and the pores and the veiled spots, extend from one pole of the sun to the other; so that whatever it may lead us to, we are bound to consider that there is the most intimate connection between spots, metallic prominences, and faculæ, and that there is a great difference between the metallic prominences and the quiet ones. That is a result to have arrived at of the first order of importance.

So far then the facts are in accord with the hypothesis, and indeed the hypothesis closely connects together phenomena which formerly seemed isolated. Thus, for instance, we find explained the close connection between metallic prominences and spots; the entire absence of metallic prominences with rapid motion from any but the spot-zones; the fact that the faculæ always follow the formation of a spot and never precede it; that the faculous matter lags behind the spot as a rule; the existence of veiled spots and minor prominences in regions outside the spot-zones; the general injection of unknown substances into the lower levels of the chromosphere which I first observed in 1871, and which have been regularly recorded by the Italian observers since that time;—all these phenomena, and many others which might be referred to, are demanded by the hypothesis, and are simply and sufficiently explained by it.

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[1] This requires a small qualification. A few have been recorded in higher latitudes, notably one observed by Peters in 1846 in N.L. 50°, and one by Carrington in July 1858 in S.L. 44°.

If then the most vividly incandescent portion of the metallic prominences follow indirectly from the fall of material, as they have been shown to do, they must be produced by violent explosions due to sudden expansions among this cooler material brought down to form the spots, when they reach the higher temperature at and below the photosphere level.

7. *By the hypothesis the effects produced must be on a scale transcending those we should expect from the action of heat on the chemical elements we are familiar with here.*

If we assume that metallic iron can exist in any part of the sun's atmosphere, and that it falls to the photosphere to produce a spot, the vapour produced by the fall of one million tons[1] will give us the following volumes:—

Temperature.	Pressure.	Volume in cubic miles.
2,000° C.	380 mm.	0·8
10,000	760 „	1·8
20,000	5 atmos.	0·7
50,000	760 mm.	8 0
50,000	190 „	35·2

If we assume the molecule of iron to be dissociated ten times by successive halving, then the volume occupied will be 1024 times greater, and we shall have—

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[1] The volume of these = ·00003128 cubic miles.

Temperature.	Pressure.	Volume in cubic miles.
50,000° C.	760 mm.	9,011
50,000	190 ,,	36,044

In these higher figures we certainly do approach the scale on which we know solar phenomena to take place; the tremendous rending of the photosphere, upward velocities of 250 miles a second, and even higher horizontal velocities according to Peters, are much more in harmony with the figures in the second table than the first.

The question really is how does a mass of hydrogen, or ot what we call as a "short title" hydrogen, ascending at the rate of 250 miles a second get its initial velocity?

In many theories of the solar constitution we have the simple explanation that it has been squirted out of solar volcanoes. But it is I think evident that such a view scarcely merits discussion. Where are the volcanoes, and how can they exist in a mass of gas?

On this point Professor Young writes as follows:—[1]

> "When we inquire what forces impart such a velocity, the subject becomes difficult. If we could admit that the surface of the sun is solid, or even liquid, as Zöllner thinks, then it would be easy to understand the phenomena as eruptions, analogous to those of volcanoes on the earth, though on the solar scale. But it is next to certain that the sun is mainly gaseous, and that its luminous surface or photosphere is a sheet of incandescent clouds, like those of the earth, except that water droplets are replaced by droplets of the metals; and it is difficult to see how such a shell could exert sufficient confining power upon the imprisoned gases to explain such tremendous velocity in the ejected matter."

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[1] *The Sun*, p. 210.

We know indeed that meteorites falling in our own atmosphere explode violently, but how this effect must be enhanced, when the fall of small masses through a low cool atmosphere is exchanged for the falls of hundreds of millions of tons through a high incandescent one, each stratum of which is hotter than the one overlying it, until at length the temperature cannot be defined! It must not be forgotten that at times the quantity of vapours produced, vapours which are truly incandescent, though they are cooler than the photosphere, is sufficient, whatever be the process of their production, to cover an area of a thousand millions of square miles.

It is a fact worth pointing out, that if we assume a million tons of iron to be dissociated so as to get the increase of volume indicated in the second table, and to explode in $\frac{1}{10}$th of a second, the initial velocity of the dissociated particles along the line of least resistance would be something like 200 miles a second.

8. *By the hypothesis, since we know from photographs of the eclipsed sun that the thickness of the corona is greatest near the equator, the nearer a spot is to the equator the more rapid should be its motion across the disc, because the forward (eastward) motion of the particles which reach the photosphere is the more rapid before they begin to descend.*

Scheiner—who was the first to observe the spots with any very great and continuous care—made what appeared to him the paradoxical discovery that the spots which were nearest to the sun's equator appeared to travel at a quicker rate than those which were nearer the sun's poles. This fact, however, was not generally recognised until much later, and the discrepancies between the various periods of solar rotation deduced by different observers, occasioned much perplexity. In reality, they depended simply upon the position on the sun's disc of the particular spot observed for the purpose. The average time in which the spots appear to cross the sun is about twenty-eight days. This is what is termed the synodic period, because the observations are made from the earth, which is moving in the same direction as the spots while the observations are being made. Making the correction for the movement of the earth, and getting the actual period, the twenty-eight days have to be brought down to something like twenty-six for an average spot. Carrington paid very great attention to this point, and to

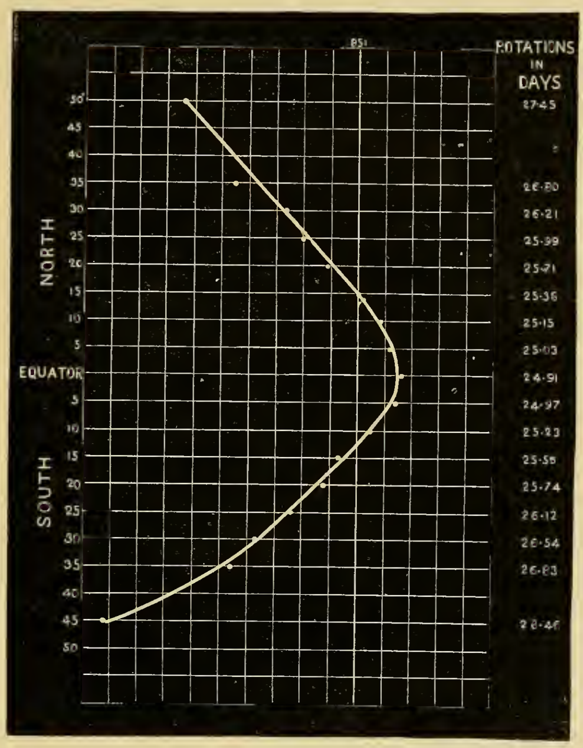

FIG. 125.—Curve showing the period of rotation of the photosphere in different latitudes north and south, from Carrington's observations; 851′ of solar longitude per diem = rate of rotation in lat. 15° N. The vertical lines represent differences of 10′ of longitude, + to the right, – to the left, of the line cutting the curve in lat. 15° N.

his work, and also to Spörer's, we owe very much of our present knowledge on this subject.

Carrington, from the observation of some thousand spots, came to the conclusion that the photosphere in which these spots are supposed to float, really moves more rapidly at the equator than it does away from it, in the manner that Scheiner had suggested, in such a way that the movement at the equator really takes place in, as near as may be, twenty-five days, perhaps a little less; but that in latitude 30° there is a slackening off of a day and a half, so that it takes a spot in latitude 30° north or south not twenty-five days, but twenty-six and a half days to make its movement right round; if we go as high as latitude 45°, we have to add on still another day, and then we find that the rotation takes twenty-seven and a half days.

Several distinguished men have endeavoured to formulate a law, a mathematical statement, by which, given the rate of spot movement in one latitude, we may determine it for all other latitudes, and several of them have very nearly succeeded in doing it; but most confess that it does not amount to much at the end of the chapter; by which I mean that the formula after all contains no physical basis.

The formulæ to which I have referred may be given in this place. In them all x = daily motion in minutes of solar longitude, and l = latitude. They are as follows:—

Carrington . . . $x = 865' — 166' \sin \tfrac{7}{4} l$
Faye . . . . . . $x = 862' — 186' \sin^2 l$
Spörer . . . . $x = 1011' — 203' \sin (41° 13' + l)$
Zöllner . . . . $x = \dfrac{863' — 619' \sin^3 l.}{\cos l}$

The second of these four expressions deserves particular attention, since it professes to be, not like the rest, merely empirical, but to convey a "law" in the true sense—that is, a rule of action with a cause behind it. M. Faye's theory of the sun's rotation is that it results from vertical convection-currents always ploughing its mass, bringing up with them from below the smaller linear velocity of the interior, and carrying down the swifter rate of travel belonging to a wider circumference. An interior solar nucleus with *accelerated* rotation is thus formed, and owing to its considerable polar compression, the currents starting from its surface rise from a greater depth, and hence carry up to the photosphere a slower rate of rotation, the farther they are from the equator. Moreover, the slackening of angular movement thus brought about will vary (as stated in the formula, and confirmed by observation) proportionally to the square of the line of the latitude.[1] It has, however, been shown that in order to produce the requisite amount of flattening in the interior nucleus, its axial revolution should be accomplished in a period of between two and three days,[2] a supposition altogether inadmissible; and indeed all attempts to account for the sun's rotational pecularities by action from *beneath* the photosphere encounter insuperable obstacles.

9. *By the hypothesis, the spots should have the greatest indications of activity, and display the greatest amount of energy on their following or western side.*

As the fall from a region of greater radius gives rise as we have seen to a forward movement in the direction of the sun's rotation, so the up-rise of the dissociated gases (produced by the masses which have pierced the photosphere and have exploded or have become dissociated below it), from below the photosphere — from a region of smaller radius — will produce a movement in the opposite direction, they should curve to the westward.

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[1] *Comptes Rendus*, t. lx. p. 147 ; t. c. p. 595.

[2] Belopolsky, *Astr. Nach.*, No. 2722.

The more profound the descent below the photosphere, the more elongated will the spot system become by the receding of the photosphere and the formation of vast ridges of faculæ to the westward.

The fact of the great disturbances lying on the following edge of a spot, was first pointed out by Messrs. De la Rue, Stewart, and Loewy, and has been confirmed by the most recent work of Spörer, who finds that the most rapid proper motions always occur on the western side. These sometimes amount to 1,000 or 2,000 miles a day, and are most intense when the largest spot-groups are formed, *and slacken down after their formation.*[1] Such groups tend, as Spörer has remarked, to extend along the parallel of latitude they originate in.[2]

10. *By the hypothesis the falls should be least at the poles and at the equator.*

We have seen (in 8) that the atmosphere over the equator is higher than it is over the poles. Working on this fact alone it is clear that near the poles the particles will have a less, and near the equator a greater, distance to fall than those in mid latitudes.

At the poles the velocity of the particles when they reach the photosphere will be relatively small; near the equator, though the velocity will be relatively great, the thickness of incandescent atmosphere to be traversed will be greater also. Both these conditions are unfavourable to large disturbances of the photosphere.

Hence those particles and masses will be more effective which fall neither at the poles nor at the equator; and in each hemisphere N. and S. there should be a zone of latitude in which the falls will be *most* effective.

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[1] *Nature*, Nov. 1886, p. 72.

[2] *Publicationen*, Potsdam, No. 1 (1878), p. 91; No. 5 (1880), p. 69.

That there are no large spots either at the poles or the equator, is one of the best known facts of solar physics.

11. *By the hypothesis the falls at any one time, especially when the sun is least disturbed, should occur in about the same latitude on either side of the equator, thus forming two zones, one north, the other south.*

This follows naturally from the symmetry of the atmosphere, *in the main,* producing equal thickness of atmosphere in the same latitudes N. and S.

The facts are quite in accord. As a rule, whatever be the latitude of the spots north of the equator at any one time, those on the south follow suit.

12. *By the hypothesis the latitudes of the north and south zones of falls cannot remain constant.*

The falls will increase the temperature of the atmosphere over the spots and prominences produced by them (see 5), and they will end by perhaps increasing the height of the atmosphere as well; the particles then will descend effectively with greater difficulty from both these causes, and their effects will cease.

There is ample evidence in eclipse photographs of these local heatings to great heights, and there is also the fact that from the first appearance of spots after a state of quiescence (a sun-spot minimum), they continually change their latitude.

This is a question of the highest importance, for the fact that the falls continue at all, shows that so far we have only considered one factor, and that there is another. To this we must next refer.

13. *By the hypothesis, if the falls continue and vary their latitude, constantly increasing or diminishing it, this can only happen because currents either from the equator in one case or from the poles in the other, are acting upon the exterior layers of condensed and condensing material.*

As a matter of fact the spots from the minimum period decrease their latitude. This is undoubted, and rests upon the observations of Carrington and Spörer; it is also amply confirmed by the Kew and Greenwich reductions of solar photographs.

The accompanying curves, which bring together the results of Spörer's observations from 1854 to 1880, show the latitudes of the spots observed in each hemisphere (they were mainly the same in both) in each year. It will be seen that in that period there were two series of spots. The first beginning in 1856 and extending with constantly decreasing latitude till 1868, and the second beginning in 1866 and lasting till 1880. We learn from this that the true sun-spot cycle is one extending over twelve or fourteen years, and that another begins in high latitudes before the former one has ceased.

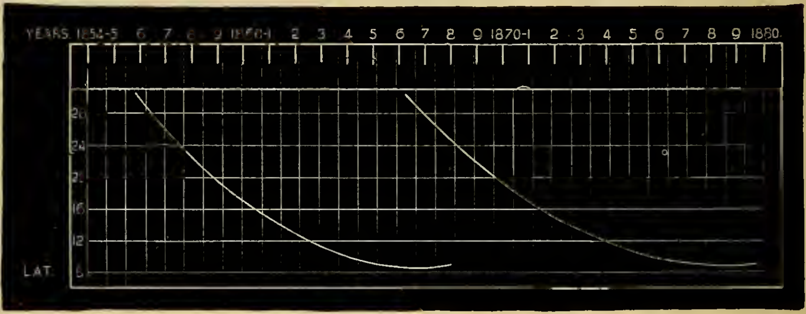

FIG. 126.—Spörer's curve of the change in the latitudes of sun-spots. Two cycles are shown, the first begins in a high latitude, and the latitudes are constantly reduced till it ceases. A second cycle begins in a high latitude before the first ceases in a low one.

Nothing could more closely indicate than these curves the fact that in both hemispheres, north and south alike, the spots are formed successively in lower latitudes, one cycle beginning before the former one is ended.

Hence we are driven by the hypothesis to assume an upper outward current from the poles. Here we find ourselves in a part of solar physics where the facts are almost entirely wanting.

With regard to these currents it must at once be pointed out that as the falls occur in different longitudes the strengths of the currents will not be the same in all longitudes. And if the currents are more intense in one longitude, and if they really do bring with them the spot-forming materials, we should expect spots, when there are fewest of them (when we can best study their action) to have a tendency to form along meridians. A discussion of the spots observed in the years 1878 and 1879 certainly shows that there was such a tendency very strongly marked.

Nor is this all. An action once set up along a meridian may be the cause of a succession of spots breaking out in the same longitude, even after some considerable interval of time. On this I quote the following from Carrington :—[1]

> "667. A group seen twice, on March 13th and 15th. Not seen on the 18th. In the next rotation 690 occupies the same position, and in the third rotation a large group (771) succeeds. There is no question that the three are successive independent formations or outbreaks in the same region. This and other cases (668 immediately before is another) indicate that the source of energy which leads to the formation of a spot or group is not always exhausted on the disappearance of the group ; that corresponding to the visible spot there is an invisible overhanging cloud or underlying volcano, the discharge of which rupturing or displacing the photosphere is sometimes intermittent."

[1] Carrington, *Observations of Solar Spots,* p. 176.

It was hoped in the eclipse of 1886 to acquire some information regarding this point, but unfortunately a cloud prevented the work specially undertaken with this object in view. Still, as early as 1875, and later, in 1878, the photographs taken in these years suggested an upper outflow from the poles; and in 1878 my own eye observations suggested this strongly to me before I had seen the photographs.

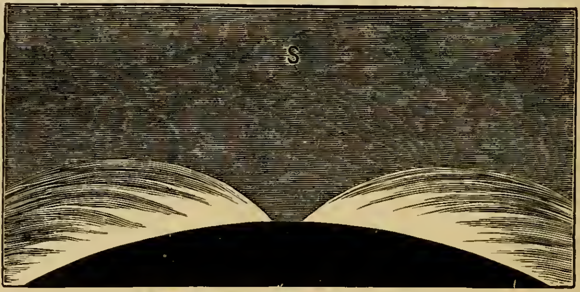

FIG. 127.—Observation of the phenomena at the sun's north pole, 1878. (Lockyer.)

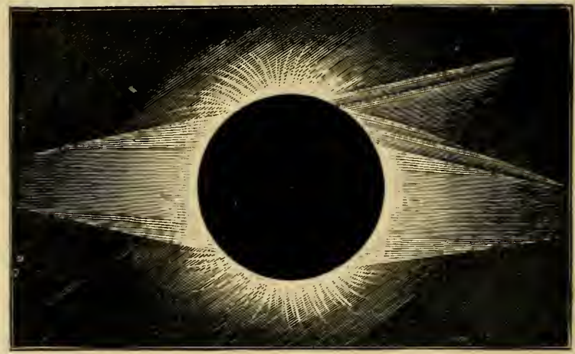

FIG. 128.—The appearances presented by the sun's poles in the American photographs.

The numerous photographs taken of the same eclipse show this much better.

An outer current carrying condensed and condensing material from the poles thus suggested, and partly confirmed by observation, necessitates an inner current towards the poles carrying the lower incandescent vapours to feed it. Here again the information to hand is meagre, but such as it is it gives no uncertain sound.

First, in the observations of prominences of no great height, strong indications of currents have been observed. So far, sufficient attention has not been given to record the direction of the drift, but this point is now engaging the attention of the Rev. S. J. Perry at Stonyhurst. The illustrations from Young's work given on p. 115 will show the way in which these lower currents act upon the prominences.

Another way of throwing light upon this still obscure question is to observe the proper motions of spots. I have done this for the years 1878—9, and find in many cases exactly what I should have expected; namely that the spots drift polewards, the lower current being opposite in direction to that which brings the falling materials. In many of these spots we can trace the paying out of the materials, and the formation of spots in gradually lower latitudes along the same meridian, and the subsequent drift of the vapours back on their paths at a lower level.

14. *By the hypothesis, the falls when they have once begun should increase their intensity very rapidly.*

As it is the falls which increase the temperature of the lower atmosphere, the more falls there are, the stronger will those currents become, which carry the dissociated material polewards, and the more material there will be to be condensed and carried by the return upper current towards the equator.

It is very evident that once such a machinery as this is set going on the solar scale, the action will go on with a rapidly increasing velocity and energy. This is in accordance with the facts.

The accompanying curves give Spörer's results of the quantity of spotted area between the years 1856 and 1880, the maximum area being taken as 100. It will be seen that there were practically no spots in 1856, the maximum was reached in 1860; again, that there were practically no spots in 1867, and the maximum was again reached in 1871.

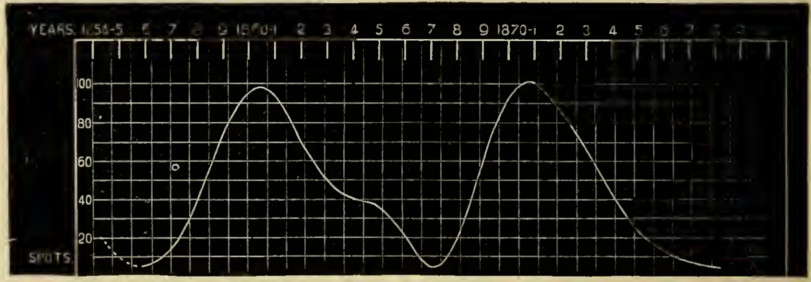

FIG. 129.—Spörer's curve, showing amount of spotted area during the sun-spot cycle.

We now come I think to one of the most interesting conclusions to which the hypothesis naturally leads us, although so far the phenomena have resisted explanation.

15. *By the hypothesis this increase in the numbers and intensity of the falls must end in so increasing the temperature and possibly the height of the atmosphere, that the descending materials are dissociated before they reach the photosphere; the production of spots therefore gradually diminishes until finally the spot producing material descends as gentle rain; the spots disappear and the cycle is ended.*

If we now study Spörer's curves side by side, we find that after the maximum spotted area is reached some four years after each cycle commences, the area diminishes gradually during the next seven years, and that the diminution is much more gentle—the curve is much less steep—than the increase.

Further, we learn that another cycle really begins, as we have already said, before the preceding one is finished; so that in the case of each cycle we have eight or ten years instead of seven after the maximum. The sun-spot period of eleven years, therefore, is the time that elapses between maximum and maximum, and is shorter than each true cycle.

The curves also show us that the maximum is reached when the mean latitude of the spots is about 16° N. and S. We get then a retreat from about 30° to 16°=14° in four years, and the further retreat from 16° to 8°=8° in eight years, that is a change of latitude of over 3° a year to begin with, and a change of half a degree a year to end with.

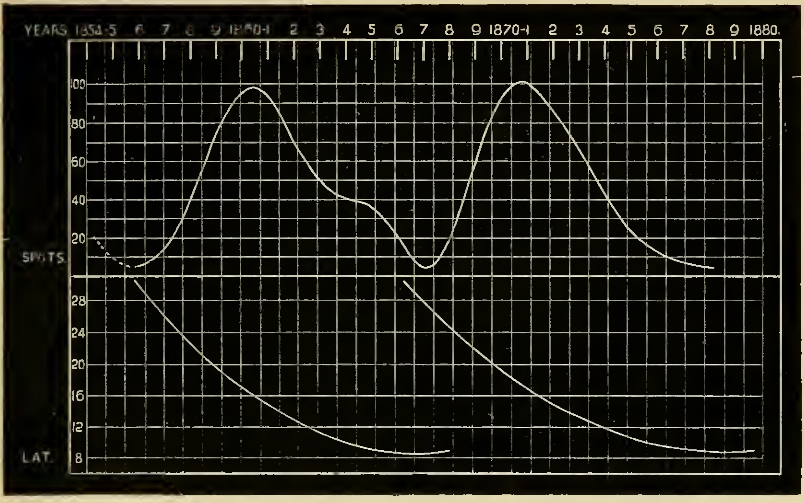

Fig. 130.—Combination of Spörer's curves.

We gather then, that after latitude 16° there is a great and increasing difficulty of fall.

To discuss this properly we must now pause and take a general view of the ground we have recently travelled over.

We have seen in (13) how the hypothesis suggests to us that the sun-spot period is a direct effect of the atmospheric circulation, and that the latitudes at which the spots commence to form at the minimum, which they occupy chiefly at the maximum, and at which they die out at the end of one period in one hemisphere, after they have commenced to form a second one, in the same or the other one or in both, are a direct result of the local heating produced by the fall of matter from above descending to the photosphere, and perhaps piercing it. The results of this piercing, are, the liberation of heat from below, and various explosive effects, which, acting along the line of least resistance, give, as a return current, incandescent vapours ascending at a rate which may be taken as a maximum at 250 miles a second, a velocity sufficient to carry them to very considerable heights.

Next we considered the evidence showing that the atmospheric circulation is kept up by upper outflows from the poles towards the equatorial regions. In these outflows a particle constantly travels, so that its latitude decreases and its height increases, the true solar atmosphere then resembles the flattened globe in Plateau's well-known experiment.

These currents, as they exist in the higher regions of the atmosphere, carry and gather the condensing and condensed materials towards the equator.

We know that when the solar forces are weakest, slight descents of material take place all over the sun, because at that time the spectrum of the corona, instead of being chiefly that of hydrogen, is one of a most complex nature, so complex that before 1882 it was regarded by everybody as a pure continuous spectrum, such as is given by the limelight.

When the fall of spot material begins in earnest we get a return up-current in the shape of active metallic prominences, and the production of cones and horns which probably represent the highest states of incandescence over large areas and extending to great heights; and, besides these, the production of streamers. Two results must follow from this which we have already referred to, viz:—

1. In consequence of the increased temperature of the lower regions, the velocity of the lower currents towards the poles, and therefore of the upper currents from the poles, is enormously increased.

2. Violent up-rushes of the heated photospheric gases, mounting with an initial velocity of a million miles an hour, will also produce local disturbances in the upper atmosphere.

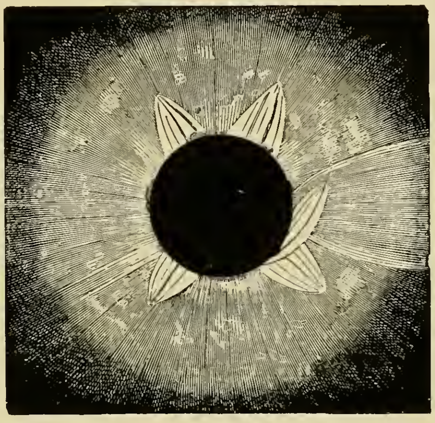

FIG. 131.—The eclipse of 1858, showing cones.

In this way, the sudden rise to maximum in the sun-spot curve, and the lowering of the latitude of the spots, follow as a matter of course from the unequal strength of the currents from the pole.

Does the hypothesis explain then the slow descent to minimum and the still decreasing latitude? It does more, it demands it. For now the atmosphere over those regions where the spots have hitherto been formed, is so highly heated, and its height is so increased, that any disturbed material descending through it will be volatilised before it can reach the photosphere. The best chance that descending particles have then to form spots, is, if they fall from points in lower latitudes. The final period, therefore, of the sun-spot curve must be restricted to a very large extent to latitudes very near the equator, and this is the fact also, as is well known.

If the reasoning so far be accepted, I should like to add that in my own mind the constant appearance of spots at the beginning of a cycle in lat. 35°-30°, and the reduction in the spotted area when latitude 10° is reached, indicate that in those regions there is some sudden change in the physics and construction of the atmosphere. In (9) we saw why, if my hypothesis be true, there should be fewer spots at the poles and equator, assuming only an increasing height of atmosphere at the equator, but the hypothesis says nothing as to why latitudes 35-30° are chosen. Now are there any facts to help us here? I think there are, but here again we are in a region of great uncertainty; we have not enough information.

If we study the records secured during eclipses observed when the sun is least disturbed, there is evidence to show that the condensing and condensed materials brought towards the equator by the outward polar current, probably extend as solar meteoric masses far beyond the limits of the true atmosphere, and form a ring, the section of which widens towards the sun, and the base of which lies well within the boundary of the atmosphere.

There is further evidence indicating that the equatorial extension shown on the photographs may only after all have been a part of a much more extended phenomenon, one going to an almost incredible distance—considering it as a solar appendage—from the sun itself.

During the eclipse of 1878, one observer took extreme precautions to guard his eyes from being fatigued by the light of the inner corona, which sometimes is so bright that observers have mistaken it for the limb of the sun itself. What this observer, Prof. Newcomb, did, was to erect a screen which covered the moon and a space 12′ high round it. The result was, that as soon as he took his station at the commencement of totality, he saw a tremendous extension along the sun's equatorial plane on both sides the dark moon, the extension being greater than that recorded in the photograph. It does not follow that the photograph gives us the totality of the extension; it may be that the extended portions may have been so delicately illuminated, that they would not impress their image on the photographic plate in the time during which that plate was exposed,

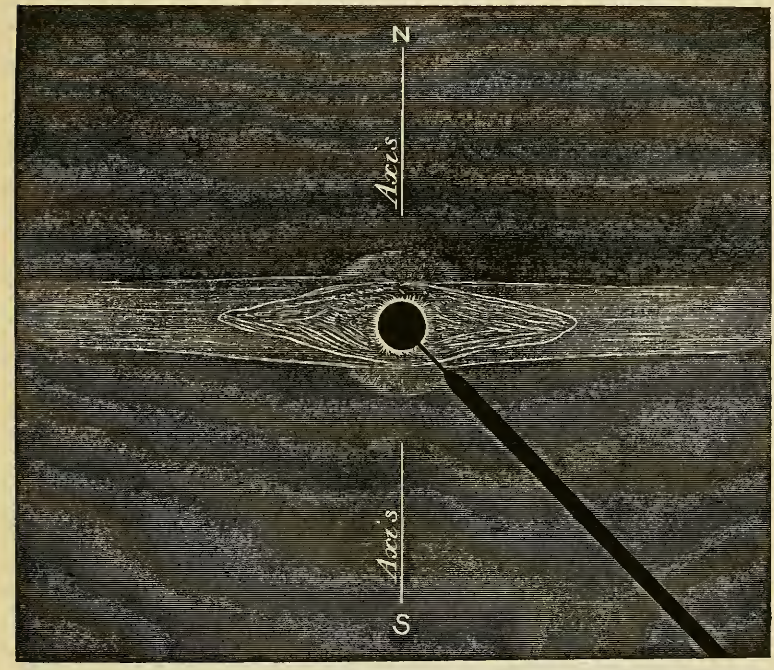

FIG. 132.—Tracing of Newcomb's observation of 1878, the brighter portion of the corona being hidden by a screen. Shows the equatorial extension and concentric atmospheres.

or that the light itself is poor in blue rays. So considerable was this extension, amounting to six or seven diameters of the dark moon, that Prof. Newcomb was inclined to ascribe it to the zodiacal light. While this observation was being made by Prof. Newcomb at a height of about 7,000 feet, other observers were viewing the eclipse from Pike's Peak, some few hundred miles away, at a height of 13,000 feet. We can imagine the purity of the air at that height; there was not too much of it—so little in fact that some observers had to go down. These saw the corona very well indeed; and one or two without taking the precaution of putting up a screen, saw an extension comparable with that recorded by Prof. Newcomb.

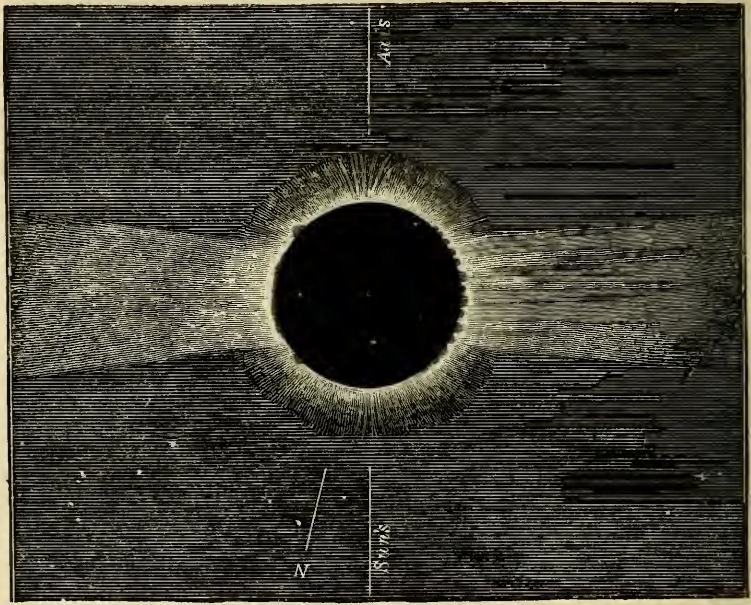

FIG. 133.—The equatorial extension and polar tracery observed at the minimum of 1867.

That, then, we must take to be the undoubted result arrived at during the eclipse of 1878, which happened at the last sun-spot minimum. We have a tremendous equatorial extension; that is the great feature, and it is proved by photographs, and this extension may mean that there is a ring round the sun's equator.

A similar extension was observed in the previous minimum, in 1867, and the polar phenomena were observed to be identical in both eclipses. At the poles there is an exquisite tracery curved in opposite directions, consisting of plumes or *panaches*, which bend gently and symmetrically from the axis, getting more and more inclined to it, so that those in latitudes 80° to 70° start nearly at right angles to the axis, and their upper portions droop gracefully, and curve over into lower latitudes.

Although indications of the existence of this ring have not been recorded during eclipses which have happened at the period of maximum, there was distinct evidence both in the eclipses in 1871 and 1875 of the existence of what I regard as the indications of outward upper polar currents observed at minimum.

If we assume such a ring under absolutely stable conditions, there will be no disturbance, no fall of material, therefore there will be no spots, and therefore again there will be no prominences. Such was the state of things on the southern surface of the ring from December 1877, to April 1879, during which period there was not a single spot observed, the umbra of which was over 15-millionths of the sun's visible hemisphere.

Assume a disturbance. This, as in the case of the atmosphere without any ring discussed in (2) page 406, may arise from collisions, and these collisions would be most likely to happen among the particles where the surface of the ring is being bombarded by the condensed materials brought by the currents from the poles. These particles will fall, thereby disturbing and arresting the motion of other particles nearer the photosphere; and finally they will descend with a crash on to the photosphere, from that point where the surface of the ring enters the atmosphere, some distance farther down.

The American photographs in 1878 supply us with ample evidence that this will be somewhere about latitude 30°, and here alone will the first spots be formed; the reasoning being the same as that employed when we considered the atmosphere alone without the ring.

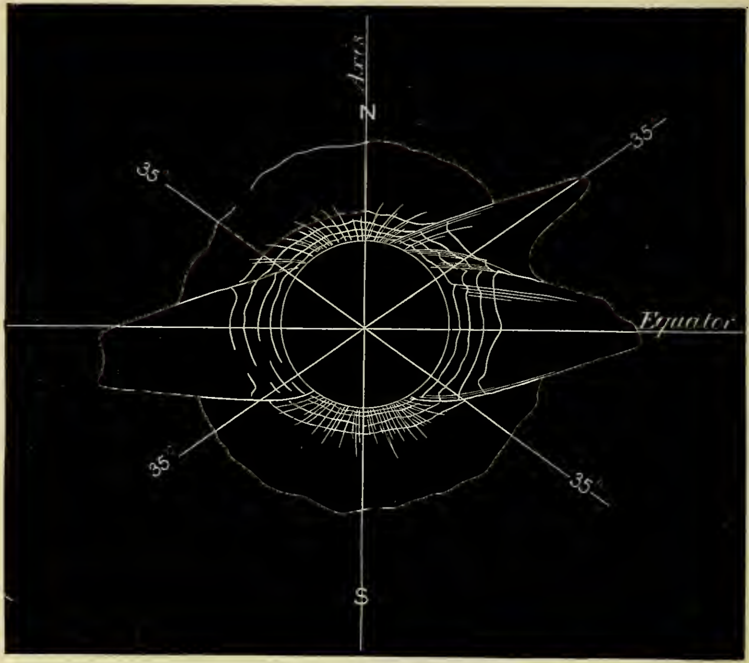

Fig. 134.—Tracing of the results obtained by the cameras in 1878, showing inner portion of equatorial extension, and how the surfaces of it cut the concentric atmosphere in lat. 35 N. and S., or thereabouts.

(1) Above latitude 30°, as a rule, we shall have no spots, because there is no ring, and further, the atmosphere is of lower elevation; so that there is not sufficient height of fall to give the velocities required to bring down the material in the solid form.

(2) In the central plane of the ring over the equator, the particles to be disturbed by the currents will be more numerous; a rapid descent, therefore, in this central plane will be impossible, for the reason that the condensed matter has to fall perhaps a million of miles through strata of increasing temperature; there will, therefore, be no spots.

The lower corona where the corona is high, and it is highest over the equator, acts as a shield or buffer, volatilisation and dissociation take place at higher levels. Where this occurs, spots are replaced by a gentle rain of fine particles slowly descending; instead of the fall of mighty masses and large quantities of solid and liquid materials, volatilisation will take place gradually during the descent, and at the utmost only a veiled spot will be produced.

It will be gathered I think then from the above that if such a ring really exists, it may have great influence in deciding the exact latitude for the outbreak of spots and the rate at which the action slows down at the equator.

16. *By the hypothesis, as the falls produce a higher temperature in the lower atmosphere; at and after the maximum the corona will be brighter, and the spectrum will be more truly a gaseous one than at and after the maximum.*

This is in strict accordance with all the observations of the last twenty years. The brightness of the corona at the minimum in 1878 was estimated at $\frac{1}{7}$ of that of the preceding maximum in the year 1870. The light of the corona was very feeble in 1886, three years from the maximum, the preceding maximum having taken place in 1883. Unfortunately I did not see the corona this year, but I saw the stars between the clouds, almost as during the night, certainly more than on a full moon night; while in 1878 from an elevation of some 7,000 feet in the Rocky Mountains, in the clearest air perhaps in the world, I saw very few.

In 1871 in India and in 1882 in Egypt, both maximum years, I did not see half a dozen on either occasion, a very striking fact, and one not explained by the varying duration of totality. Of course the longer the totality, the more is the coronal light as well as the photospheric light prevented from reaching the atmosphere.

17. *By the hypothesis the solar radiation will be greatest when the number of falls is greatest.*

It is a result of observation that from the time of the first falls in a latitude above 30°, down to the end of the cycle in latitude 6° or 8°, a period of something like thirteen or fourteen years elapses. If this solar cycle depends upon an influence from without, this external influence must therefore have a period of variation of thirteen or fourteen years. But the period effective in producing changes in the amount of solar radiation to such a body as the earth, must depend, if the changes exist, upon the time which elapses between successive maxima, that is, about eleven years.

In considering this question of radiation it must be borne in mind, that the more the excess radiation is localised, as, say in the spot zones, the more effective it will be; as the smaller the radiating area of high intensity is, the less absorption will be experienced in the solar atmosphere.

That the effects of such variations in the amount of energy radiated to the Earth have already been traced in various branches of magnetical and meteorological science, is by some still considered doubtful. This matter, however, is one of such high practical importance that solar physics in their entirety would be worth investigating if more light on this point only were the sole result that might be anticipated.

It is now time to sum up this chapter. It will have been seen that the idea of dissociation is not out of harmony with any known solar phenomena, and that its introduction renders a reason for many of them which up to the present time have resisted all attempts at explanation. I may go further and say, as I have said before, that it demands those very phenomena which proved stumbling-blocks to so many investigators, so long as it was supposed that Solar Chemistry was conditioned similarly to our own.

When the wonderful chemical changes which take place in the sun from cycle to cycle are more widely known, the importance of basing our views of the solar economy on both the chemical and physical facts available will be more generally recognised.

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